PROCEEDINGS

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PROCEEDINGS

OF

THE ROYAL SOCIETY.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On a Portable Ophthalmometer." By Thomas Reid, M.D. Communicated by Lord Kelvin, P.R.S. Received January 1, 1893.

The object of the instrument about to be described is to measure the curvature of the central area of the cornea, the polar or optical zone; or of any spherical reflecting surface of from 6 to 10 mm. of radius. In its present form the instrument can only be applied to the measurement of the corneal surface in the visual line. As this is the area of the cornea utilised for distinct vision, this instrument furnishes all the data practically requisite for the diagnosis and measurement of corneal astigmatism.

The theory of its construction is based on a particular application of the following well-known optical law: that when two centred optical systems are so combined that their principal foci coincide, the ratio of the size of the object to the size of the image formed by the combined systems is equal to the ratio of the principal foci of the two optical systems, adjacent respectively to object and image. The two optical systems in this case are the convex lens of the instrument and the cornea as a reflecting surface, with the object in the principal focus of the adjoining optical system.

Thus :—(*vide* fig. 1).

Let MM' be the convex lens of known focus, A the corneal surface, and P' the point where their principal foci coincide.

Fig. 1.
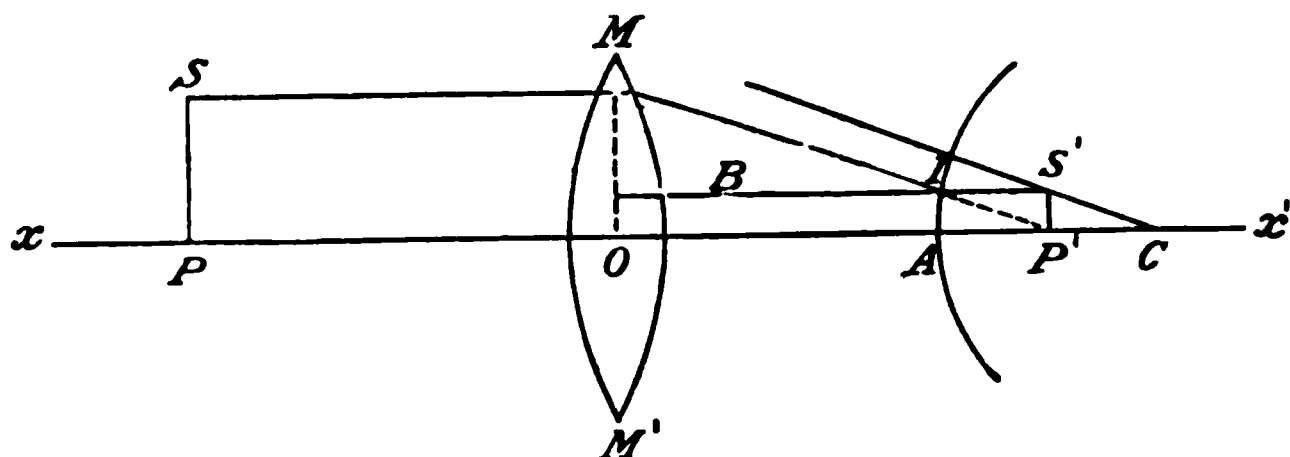
Let SP be an object situated at the principal focal distance of MM', and let XX' be the principal axis of the system.

Then a ray SM parallel to the axis will, after refraction, be directed to the principal focus P' of the curved surface of the cornea, and therefore be reflected in the direction IB parallel to the axis XX'. IB prolonged will meet the ray directed to the centre C at the point S'; therefore S' is the image of S, and S'P' the image of SP, and S'P' is in the principal focus of the convex reflecting surface.

In the similar triangles MP'O and IP'A,

$$\frac{P'O}{P'A} = \frac{MO}{IA},$$

SM and IS', the prolongation of the reflected ray, are parallel to the axis XX',

therefore $\qquad$ $SP = MO$ and $S'P' = IA.$

Therefore $\qquad$ $\dfrac{P'O}{P'A} = \dfrac{SP}{S'P'}$ or $\dfrac{O}{I} = \dfrac{F}{f}.$

$$f = \frac{r}{2};$$

therefore $\qquad$ $\dfrac{O}{I} = \dfrac{2F}{r}$ .... (I);

therefore $\qquad$ $r = \dfrac{2F \times I}{O}.$

Description of the Instrument.

The essential parts of the instrument are an aplanatic convex lens of known focus, a rectangular prism neutralised in its centre by a smaller prism, one side of the rectangular prism being adjacent to the lens and a circular or other disc being opposite the other side in the principal focus of the lens. When the instrument is held in front of the convex reflecting surface with the disc turned towards a luminous

source, a virtual image of the disc will be formed at the virtual focus of the convex reflecting surface. This image will only be seen distinctly by the emmetropic eye through the neutralised portion of the prism, when the focus of the lens in front coincides with the virtual focus of the convex surface. The ratio of the object to the image will be as shown above. If now a double-image prism be inserted behind the neutralising prism, which exactly doubles this image, its power with the combination is easily determined, and therefore the exact size of the image can be measured. The size of the object being known, we have the three elements necessary for determining the fourth proportional, the curvature of the convex reflecting surface.

The instrument in this simple form presented a number of practical difficulties in its manipulation, which were overcome by the introduction of a short telescope behind, with double-image prism fixed in front of its object-glass.

Fig. 2.
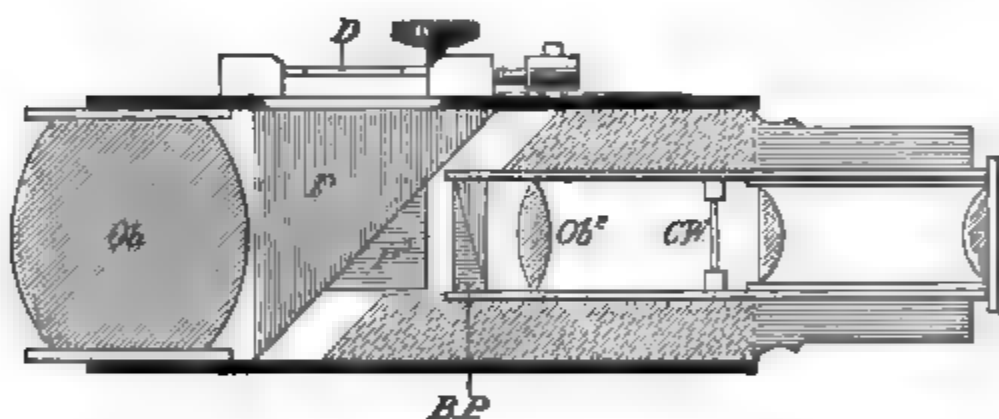
In its present form the instrument consists of the following parts (*vide* fig. 2*) :—An aplanatic lens Ob, a rectangular prism P neutralised in the visual axis by a smaller prism P² and a telescope, with the double-image prism BP fixed in front of the object glass of the telescope Ob². The focal length of the object-glass Ob² is precisely the same as that of the aplanatic lens Ob, and cross wires CW at its principal focus are viewed by a Ramsden eye-piece.

Before using the instrument it is necessary and sufficient that the cross wires should be distinctly seen at the punctum remotum of the observer. The adjusted instrument is held in the observer's left hand, which rests on the forehead of the patient, the disc being directed to a luminous source to the right of the observer. The point of coincidence of the principal foci is found by moving the instrument to and fro. When the observed eye is directed to the central or

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* The object-glass has been specially constructed according to the formula of Professor Abbe, and contains barium glass as one of its constituents.

fixation point and his visual line is vertical to the point of the cornea through which it passes, the corneal image doubled and inverted ought to be seen in the centre of the field. Instead of using circular discs of different dimensions, the size of image required to produce exact contact in any meridian is conveniently and quickly obtained by making the required change in the size of a carefully constructed iris diaphragm. By using a circular object, the circular, elliptical, or irregular form of the image reveals at once the condition of the surface. When the image is elliptical, the meridian of greatest curvature is easily found by rotation of the telescope, and a rotation of 180° gives a controlling observation. By a similar process the meridian of least curvature is determined.

Graduation of the Instrument.

Let D be the power in dioptres of the cornea as a refracting surface, with a medium behind it of uniform density having an index of refraction $n = 1\cdot337$ approximately.

$$D = \frac{(n-1)\,1000}{r}$$

$$= \frac{337}{r} \quad \dots\dots\dots\dots\dots\dots\dots\dots\dots \quad (II).$$

Combining equation (I) with (II),

$$D = \frac{337 \times O}{2\,IF},$$

$$D+1 = \frac{337 \times O'}{2\,IF},$$

$$1 = \frac{337}{2\,IF}\,(O'-O).$$

In the present instrument I = 2, and 2F = 52,

therefore $\qquad\qquad 1 = 3\cdot24\,(O'-O),$

$$\frac{1}{3\cdot24} = O'-O;$$

therefore $\qquad\qquad$ 1D = rather less than ⅓ mm.

The index is divided into two parts, outer and inner. The outer registers the size of the image, and the inner the corresponding dioptres.

The degree of refinement with which the measurements may be carried out depends entirely on the degree of exactness of determina-

tion of the constants, especially I and F. I has been determined
exactly to 1/500th inch, and can be estimated to about 1/1000th.
The focal length of the object-glass can ·be determined by Cornu's
method, but in general it is more convenient to measure two curved
surfaces whose radii are exactly known, and within the limits of the
instrument.

The index error is found by taking the number of dioptres at suffi-
ciently great intervals within the limits of the instrument. In this
instrument, if we take the extremes of the index, $0 = 12$ mm. and
$0 = 16$ mm., we find the corresponding dioptres are 38·9 D and
51·84 D. The index being graduated in thirds of a millimetre, the
index error of each division is nearly 0·08 D, which is positive.

If the double prism be now removed, the image being single, and
the pupillary opening generally distinctly visible, it affords a means
of determining whether the visual axis passes through the centre of
the pupil.

It will be seen that this instrument differs from the ophthalmo-
meter of Helmholtz, the most perfect instrument theoretically and
practically which has been devised for this purpose, in which, while
the object is constant, the image varies with the curvature of the
surface, but always covers the same angular interval of the surface.
It resembles the *ophthalmomètre pratique* of Javal and Schiötz, in
which the doubling is effected by means of a double-image prism in-
serted between two achromatic lenses of equal focus, so that while
the image is constant the object is made to vary. With this instru-
ment, when the difference of curvature of the principal meridians is
considerable, amounting to 3 or 4 dioptres, in order to obtain
approximately accurate results it is necessary to insert birefractive
prisms of different powers, giving images of from 1 to 3 mm. In
the present instrument the image of 2 mm. has been selected as giving
sufficiently accurate results for most practical purposes, measuring
with precision, as it does, a difference of refraction of half a dioptre.
For cases outside the limits already referred to (6 to 10 mm.) prisms
of suitable powers can be substituted.

II. " The Value of the Mechanical Equivalent of Heat, deduced from some Experiments performed with the view of establishing the Relation between the Electrical and Mechanical Units, together with an Investigation into the Capacity for Heat of Water at different Temperatures." By E. H. GRIFFITHS, M.A., Assistant Lecturer, Sidney Sussex College, Cambridge. Communicated by R. T. GLAZEBROOK, F.R.S. Received January 19, 1893.

(Abstract.)

The paper of which this communication is an abstract gives the particulars of an investigation which was commenced in the year 1887, and extended to the close of 1892. The object of the enquiry is sufficiently indicated on the title page.

The values of the mechanical equivalent obtained by Joule in his later determinations differ amongst themselves by as much as 1 per cent., and the differences amongst the results obtained by succeeding observers are, with the exception of Rowland's in 1880, still greater. The harmony amongst the values obtained by Rowland is marvellous; but, since his manner of investigation was the same throughout the whole series of his experiments, his conclusions stand in need of confirmation by different methods of observation. Other observers who have attempted to obtain the value of the mechanical equivalent, by means of the work done by an electric current, have been hampered by constant perplexities as to the absolute values of the electrical units adopted. The science of electrical measurements has now arrived at such a stage that its units may be regarded as sufficiently established,[*] and, therefore, the time seems particularly appropriate for an enquiry into the relation between those units and the mechanical ones.

The difficulties of such an investigation are, of course, great, as is shown by the discrepancies amongst the results obtained by those observers who, in recent years, have adopted electrical methods. One cause of inaccuracy has been present in all determinations I have examined, viz., the increase in temperature of the conductor above the temperature of the medium in which it was placed, and the consequent undetermined alteration in its resistance. Rowland[†] writes as follows: " There can be no doubt that experiments depending on the heating of a wire give too small a value of the equivalent, seeing that the temperature of the wire during heating must always

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* ' B. A. Report,' 1892.
† ' Proceedings American Academy,' June, 1879, p. 153.

be higher than that of the water surrounding it, and hence more heat will be generated than there should be."

A short account is given in this summary of the manner in which this difficulty has been overcome, and I think it will be seen that this objection to the electrical method of investigation is now removed.

I have defined the thermal unit as the quantity of heat required to raise unit mass of water through 1° C. of the air thermometer at 15° C., and so much confusion has arisen from ambiguity as to the value of the unit, as ordinarily defined, that I have given reasons in support of the suggestion that this definition should be generally adopted.

Throughout the whole of this enquiry I have been ably assisted by Mr. G. M. Clark, B.A., Sidney College, Cambridge, and this communication should, in justice, be regarded as a joint contribution.

The value of an investigation of this kind depends, in a great measure, on the attention given to matters of detail. It is, therefore, impossible, in a short abstract, to produce the evidence on which our results are based, and we content ourselves with a brief outline of the method adopted and the conclusions arrived at, without attempting to justify those conclusions.

If a calorimeter is suspended in a chamber, the walls of which are maintained at a constant temperature, we can, by observations over a *small* range across that outside temperature, deduce the rate of rise due to the mechanical work done in the calorimeter, when the supply of heat is derived from stirring only. By repeating the observations in a similar manner over ranges whose mean temperature θ_1 differs from that of the surrounding walls θ_0, we obtain the change in temperature due to the combined effects of the stirring, radiation, conduction, and convection at all points of our whole range of temperature. As the success of the method depends (1) on the possibility of maintaining the exterior temperature unchanged, and (2) on the regularity of the supply of heat due to the stirring, we briefly indicate our method of securing those conditions.

1. The calorimeter* was suspended within an air-tight steel chamber. The walls and floor of this chamber were double, and the space between them filled with mercury. The whole structure was placed in a tank containing about 20 gallons of water, and was supported in such a manner that there were about 3 inches of water both above and beneath it. The mercury was connected by a tube with a

* The calorimeter was of cylindrical form, and suspended by three glass tubes. It was made of "gilding metal," which both internally and externally was covered with a considerable thickness of gold. All metal surfaces within the calorimeter were thickly gilded.

gas regulator of a novel form, which controlled the supply of gas to a
large number of jets.   Above those jets was placed a flat silver tube,
through which tap water was continually flowing into the tank, all
parts of which were maintained at an equal temperature by the rapid
rotation of a large screw.   Thus, the calorimeter may be regarded as
suspended within a chamber placed in the bulb of a huge thermometer
—the mercury in that bulb weighing 70 lbs.   A change of 1° C. in
the temperature of the tank water caused the mercury in the tubes of
the regulating apparatus to rise about 300 mm.   Special arrange-
ments were made by which it was possible to set the apparatus so
that the walls surrounding the calorimeter could be maintained for
any length of time at any required temperature, from that of the tap
water (in summer about 13° C., in winter 3° C.) up to 40° C. or 50° C.
We know by observation that the temperature of the steel chamber
(when once adjusted) did not vary by 1/500° C., and we believe the
variations were much less.

2. We experienced great difficulty in devising a suitable form of
stirrer; and we attribute the failure of our earlier experiments to
defects in the ordinary forms.   We find it impossible, without a
lengthy description, to give a clear idea of the stirrer ultimately
adopted.   We can only state here that it was completely immersed
when the depth of the water exceeded 1 cm., that its bearings were
outside the steel chamber, and that the water was thrown from
the bottom to the lid of the calorimeter.

More than 100 experiments were performed (many of them last-
ing several hours) in order to determine the value of $\sigma + \rho (\theta_1 - \theta_0)$,[*]
when the calorimeter contained different masses of water.   The
harmony amongst the results was satisfactory.

These experiments proved that over our range of temperature,
$\sigma + \rho(\theta_1 - \theta_0)$ was a linear function of $\theta_1 - \theta_0$, and Newton's law of
cooling appeared to hold strictly true over a range of 6° C. below
to 6° C. above the temperature of the surrounding walls, *i.e.*, from
14° C. to 26° C.; and our experimental results were of such a nature
that a very small departure would have been apparent.[†]

We found that with our form of stirrer $\sigma = r^3k$ where r was the
rate of revolution, and k some constant.   This relation held true for
all values of r between 26 and 34 revolutions per second, and, as
during our J experiments we proposed to maintain a rate of as
nearly as possible 30 revolutions per second, we were able to make
the necessary correction for small deviations from the normal rate.

In order to diminish the irregularities in the motor, a special

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[*] σ = rise in temperature per 1 second due to the stirring. ρ = gain or loss in
temperature per 1 second due to radiation, &c., when $\theta_1 - \theta_0 = 1°$ C.

[†] If the temperatures were reckoned on the mercury thermometer scale, the
currature would be considerable.

form of pressure regulator was constructed, and found to be fairly successful, the variations in r during an experiment being generally very small.

The pressure in the space between the calorimeter and the walls of the steel chamber was reduced, as a rule, to between 0·3 and 1·0 mm.*

If M is the capacity for heat of the calorimeter and its contents, Mρ will be the quantity of heat lost or gained per second by radiation, &c., per unit difference of temperature, and provided the pressure is unaltered, the value of Mρ should be constant whatever the weight of water. It was not until the close of our work that we were able to obtain the value of M. We then found that the value of Mρ varied greatly with small changes of pressure, and our results (although not necessary for the purposes of our investigation) are interesting, since they bear out Bottomley's conclusion† that there is a sudden decrease in the loss by radiation when the pressure falls below 0·5 mm. We extract from our paper the following table :—

Table XXVI.

Date.	Experiments.	Mass of water.	Pressure in mm.	Thermal grams per second.
September 10—13.....	83—93	139·78	1·15	0·0140
August 8—10	41—50	103·01	1·15	0·0140
„ 11	51—54	103·01	0·98	0·0140
September 14—16.....	94—102	199·67	0·48	0·0138
„ 16—18.....	103—110	259·50	0·48	0·0138
August 26...........	79—80	277·93	0·44	0·0136
„ 14—16	58—59	188·07	0·40	0·0134
„ 24, 25	62—65	188·07	0·37	0·0032
„ 30, 31	74—78	277·93	0·37	0·0132
„ 26, 27	66—71	277·93	0·37	0·0131
„ 17, 18	60—61	188·07	0·37	0·0131
September 4.........	81—82	140·27	0·36	0·0130

The critical point of the curve deduced from the above table occurs at a higher pressure, and the bend is somewhat sharper than is the case with the curve given by Bottomley.

If $\left(\dfrac{\delta\theta_1}{\delta t}\right)_{\sigma,\rho}$ is the rate of rise due to the non-electrical supply, and $\left(\dfrac{\delta\theta_1}{\delta t}\right)_e$ that due to the electrical supply,

then

$$\frac{\delta\theta_1}{\delta t} = \left(\frac{\delta\theta_1}{\delta t}\right)_e + \left(\frac{\delta\theta_1}{\delta t}\right)_{\sigma,\rho} \quad \ldots\ldots\ldots\ldots\ldots\ldots (1).$$

* The pressures were ascertained by a McLeod's gauge.
† 'Phil. Trans.,' 1887, A.

We have described the manner in which we determined the last term of this equation, and thus, by direct observation of $\frac{\delta\theta_1}{\delta t}$, we were able to obtain the value of $\left(\frac{\delta\theta_1}{\delta t}\right)_\epsilon$ and

$$\left(\frac{\delta\theta_1}{\delta t}\right)_\epsilon = \frac{E^2}{J.R'.M'} \quad\dots\dots\dots\dots\dots\dots (2),$$

where R' is the resistance of the coil, and M' the capacity for heat of the calorimeter and its contents at a temperature θ_1.

We have now to indicate the methods by which we ascertained the electrical value of the energy supplied by the current.
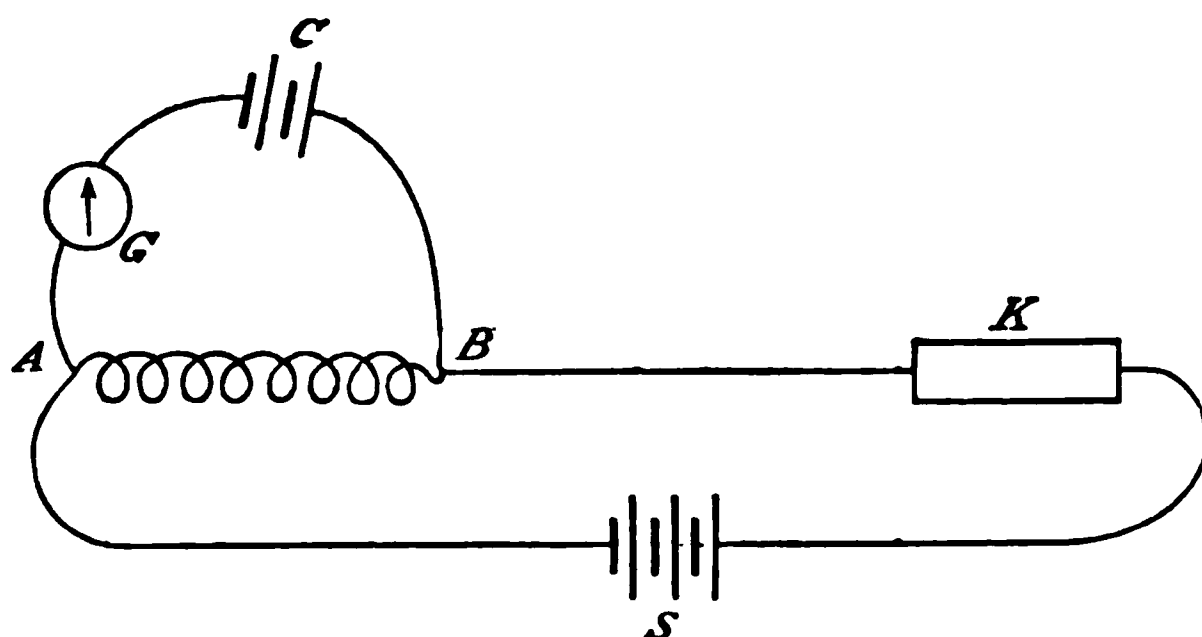
E.—The extremities of the wires CA, CB will (when the galvanometer G shows no deflection) be maintained at a difference of potential equal to that due to the cells at C.

Let K be an adjustable resistance placed in the circuit which communicates with S (the storage cells), and let R be the resistance of the calorimeter coil AB. Whatever variations may take place in R and in the potential difference at S, it is always possible (provided the adjustment of K is sufficiently under control) to maintain, by close attention to the indications of the galvanometer, the points A and B at a constant difference of potential.

A special rheochord was designed by means of which it was found possible, in spite of variations in the resistance of AB, to maintain the potential difference unchanged throughout each experiment, and we believe that in no case did the variations exceed 1/10,000 of the mean difference of potential during each experiment.

The Clark cells (which were placed at C) were constructed by us according to the directions of Messrs. Glazebrook and Skinner, and have, on several occasions, been directly compared with the Cavendish, and, indirectly, with the Berlin, standard. Their differences from the

standard are small, and their mean E.M.F. at 15° C. = 1·4344 volts.*

R'.—Had it been possible to maintain a constant value for R, it would have greatly simplified both the experimental work and the calculations. In the year 1890 we devoted much time to the examination of the various copper-manganese-nickel alloys, and we performed a series of determinations with a coil whose temperature coefficient was practically zero. The reasons which led us to reject these alloys and adopt a platinum wire will, we think, be found sufficient.

The value of R was first determined by a dial-box (legal ohms) constructed by Messrs. Elliott.† Mr. Glazebrook has been so kind as to perform a complete standardisation of this box by means of the B.A. standards. The resulting corrections have been applied, and the values of R expressed in true ohms, as defined by the 'B.A. Report,' 1892.

If R is the resistance of the coil‡ when at the standard temperature θ, then $R' = R\{1 + \kappa(\theta_1 + \beta - \theta)\}$, where κ is the temperature coefficient of the wire and β is the excess of its temperature above θ, the temperature of the calorimeter. It is difficult to describe in a few sentences the manner in which we determined the value of β, but the following explanation may serve to indicate the method of procedure.

Suppose P, Q, R, and S to be the arms of a Wheatstone's bridge of which S is the coil. Let the arms P and Q be equal, not only in resistance but in mass and dimensions, and let R = S when the coil is at a certain temperature θ_s, the reading of the thermometer in the calorimeter being θ_1. Let R be built up of a large mass of metal having a small temperature coefficient and a considerable cooling surface.§ If the bridge is balanced when a certain current is passed through it, the balance will be destroyed if the temperature of the coil S be raised as the current is increased, for the increase in temperature of R may be neglected, and P and Q will remain equal, however their values alter, since they are traversed by equal currents and their capacities for heat are the same. Equilibrium can, however, be restored by cooling the calorimeter to a certain temperature,

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* A full description of these cells will be found in Messrs. Glazebrook and Skinner's paper ('Phil. Trans.,' 1892, pp. 622—624).

† Particulars of this box have been given in a previous paper ('Phil. Trans.,' A, 1891, p. 44).

‡ The wire had a thin coating of amber varnish, and the insulation appeared to be sufficient. In order to test this, a series of observations of R were taken when the calorimeter was filled with pure pentane. The increase in R did not exceed 1 in 22,000.

§ *The mass of German silver used by us in the arm R weighed several pounds and contained about 1800 feet of single wire in triple and double strands.*

say, θ_2. We then know that S has resumed its former value, and is therefore again at the temperature θ_s; thus the value of $\beta = \theta_s - \theta_2$. By changing E, the potential difference of the ends of the coil, we can find values of β corresponding to values of E. By applying to the ends of the coils the potential balance previously described, the potential difference at the extremities of S can, by increasing the E.M.F. at the ends of the bridge, be raised to that of 1, 2, &c., Clark cells.

The following table gives the results obtained by this method. During our observations, the rate of stirring was the usual one, but we also investigated the effect of small changes in the rate. The last column gives the values deduced from the parabola $\delta R = 0.00422n^2$ (where n is the number of Clark cells), as we found it convenient to express the differences in temperature by the corresponding differences in R. A difference of 0.0010 between the last two columns corresponds to a change of 1 in 8600 in R.

The correction is a most important one, and the neglect of it by previous investigators sufficiently accounts for their failure to obtain satisfactory results by observation of the heat developed in a wire by an electric current.

Table IX.

The following numbers were plotted.		δR (legal ohms).	δR deduced from $\delta R = 0.00422n^2$.
No. of cells (n).	Increase.		
0	$-x$	0	0
1	0	0.0042	0.0042
2	0.0120	0.0163	0.0168
3	0.0333	0.0376	0.0378
4	0.0638	0.0681	0.0675
5	0.1023	0.1066	0.1055
6	0.1478	0.1516	0.1519

t, *Time.*—An electrical clock with a seconds pendulum was used as our standard for time. It was carefully compared at intervals with a chronometer by Dent. A chronograph was controlled by this clock. The rate of the clock was a losing one until August 21, but after that date its error was less than 1/25,000, and no correction was necessary.

w, *Mass.*—A balance sensitive to a change of 1/100,000 of the least mass measured by us and a set of Oertling's weights* were used in our determination of mass.

* *These weights were re-standardised by Messrs. Oertling in August, 1888.*

θ_1, *Temperature.*—At the time of writing (December, 1892) our thermometry is based on measurements made by platinum thermometers. We propose to make, at an early date, a direct comparison of our standard thermometer with the air thermometer by means of the apparatus described by Mr. Callendar,[*] who has been so kind as to promise his assistance.

In a previous paper[†] the details of a careful comparison of the platinum and the air thermometer have been given. It was then shown that the platinum-air difference curve $\delta(t|\overline{100}|^2 - t|100)$ gave values of t at all temperatures from $0°$ to $100°$ C. accurate within $0·01°$ C. of the real value of t, and that discrepancies appeared to have an experimental origin. The experience of two years' work with platinum thermometers has but increased our confidence in them.[‡] Should a direct comparison with the air thermometer modify our determinations of differences of temperature (and it is only differences which are important to us), our results will be modified accordingly. This will not, however, necessitate any repetition of the experimental work, as the corrections involved will be of a numerical order only.

A mercury thermometer by Hicks (labelled E_m[§]) was standardised by direct comparison with different platinum thermometers, the observations being taken under conditions similar to those prevalent during our "J experiments," and the temperatures corresponding to the readings given in column 1, Table XVII, were thus ascertained.

We found it necessary to calibrate the mercury thermometer not only for irregularities in the bore, but for certain strictly recurrent changes in "lag," which we have found to be invariably associated with a rising mercury thermometer. We are unable to briefly describe the method adopted, and we confine ourselves to stating that it was based on observations of time. The results of this investigation, which extended over several months, prove that a calibration of the ordinary kind is insufficient if accurate observations have to be obtained with a rising mercury thermometer. As an illustration we give the following table, which shows the difference in the value of certain ranges on thermometer E_m when steady and when rising at the normal rate of our experiments.

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[*] 'Roy. Soc. Proc.,' January, 1891.
[†] See 'Phil. Trans.,' 1891, A, p. 155.
[‡] 'Phil. Mag.,' December, 1892.
[§] *The stem of this thermometer was graduated in* mm.: about 40 mm. = 1° C.

Table XVII.*

Range E$_m$.	Range C. Thermometer steady.	Range C. Thermometer rising.
87·5—127·5	0·975	0·973
127·5—177·5	1·233	1·235
177·5—217·5	0·989	0·987
217·5—257·5	0·993	0·995
257·5—297·5	1·016	1·016
297·5—347·5	1·278	1·278
347·5—397·5	1·281	1·282
397·5—447·5	1·306	1·307
447·5—507·5	1·601	1·598
507·5—537·5	0·809	0·811

We have now indicated how the various quantities in equation (2) were determined, with the exception of J and M'; we can therefore deduce from equation (2) the time (T) of rising 1° C. at any point of our range when R = 1w and E is the potential difference of one Clark cell.

We thus get

$$\frac{J}{E^2} M^1 = T \dots\dots\dots\dots\dots (3).$$

If w be the weight of water, and w_x the water equivalent of the calorimeter at the standard temperature, and if f and g be the temperature coefficients of their specific heats, then

* [*Note, February* 18, 1893.—On February 14, ult., I received a mercury thermometer (by M. Tonnelot) which had been under observation by Dr. Guillaume for the previous two months. I have made a direct comparison of this thermometer with E$_m$ at three points, observing all the precautions enjoined by Dr. Guillaume, and the results are as follows :—

Reading E$_m$, thermometer steady.	θ_1 by Paris hydrogen thermometer.	θ_1 as given by our platinum thermometer.	Range, Paris standard.	Range from Table XVII, *supra*.
87·5	13·975	13·990	} 6·483	6·484
347·5	20·458	20·474		
507·5	24·641	24·662	} 4·183	4·188

Thus, although we differ by 0·015° C. in actual elevation at 14° C., our agreement as to the value of the above ranges is close. Want of time has prevented a complete comparison, and the observations have been somewhat hurried. They serve, however, to indicate that our thermometric errors are small.—E. H. G.]

$$M^1 = w(1 + f\overline{\theta_1 - \theta}) + w_x(1 + g\overline{\theta_1 - \theta}) ;$$

hence $$\frac{J}{E^2}\{w(1 + f\overline{\theta_1 - \theta}) + w_x(1 + g\overline{\theta_1 - \theta})\} = T \quad \ldots \ldots (4).$$

By repeating observations with different weights of water, w_1 and w_2, and observing T_1 and T_2, the corresponding times, we obtain by sub-traction

$$\frac{J}{E^2}(w_2 - w_1)(1 + f\overline{\theta_1 - \theta}) = T_1 - T_2 \quad \ldots \ldots \ldots (5).$$

Hence when $\theta_1 = \theta$ (*i.e.*, at the standard temperature) we can find J without first ascertaining the values of f, g, or the water equivalent of the calorimeter, and by repeating the observations over different ranges we can find f without previously obtaining J; or, having obtained f, we can find w_x and g, and then by equation (4) deduce the value of J from a single experiment. We have adopted both methods as a check upon the calculations, which involve much arithmetic. The latter method is the more convenient, as it enables us to ascertain the results of separate experiments, but it cannot be applied until the values of f, g, and w_x have previously been obtained by observations on two different weights at two different temperatures.

The following table shows a few of the results given in Table XL of our paper. We have divided our experiments into Series I and II, and we have given reasons why more weight should be attached to the latter series. We here give a summary of the values of T deduced from Series II. By "group" we denote all experiments conducted with the same weight of water, and in every case a group contains experiments performed with different values of n (where n is the number of Clark cells which determine the potential differ-ence). As n was in some cases changed from 2 to 6, the rate of production of heat was increased 9 times. The agreement amongst the results of experiments performed with different values of n is not shown in the portion of the table here given, but it is very close, and affords a satisfactory proof of the accuracy of the values assigned to σ and ρ, and the validity of the method employed to ascertain the actual temperature of the coil.

The number of experiments performed in each group is shown by the figure under the heading "mean." The extent of our experi-mental irregularities is clearly indicated by this table.

The "smooth curve" was in each case so drawn that the sum of the positive and negative areas included between it and the slightly irregular experimental curve (given by the numbers in the columns headed "mean") was zero.

Table XL.—Values of T (results of Series II only).

Temp.	Group A. w = 139·776.		Group C. w = 199·674.		Group D. w = 259·500.	
	Mean (6).	From curve.	Mean (4).	From curve.	Mean (5).	From curve.
14·477	458·7	458 8	580·7	580·9	702·7	702·9
15·581	459·1	458·9	581·0	581·0	703·3	703·0
16·682	459·1	459·0	581·1	581·1	703·0	703·0
17·683	459·0	459·1	581·3	581·1	703·1	703·0
18·688	459·4	459·2	581·3	581·2	703·1	703·0
19·835	459·3	459·3	581·9	581·3	703·6	703·1
21·115	459·4	459·5	581·1	581·4	703·8	703·1
22·409	459·7	459·6	581·6	581·4	702·9	703·1
23·862	459·7	459·7	581·1	581·5	702·7	703·2
25·006	459·9	459·8	581·5	581·6	703·4	703·2
No. of col. } I	XIV	XV	XVIII	XIX	XXII	XXIII

The values of T at integral values of the temperature over our range were read off from the smooth curves. We give the values at 15°, 20°, and 25° C.

Table XLI.—Values of T at 15°, 20°, and 25° C.

Temp.	Series I.		Series II.		
	Group B. w = 188·065.	Group E. w = 277·931.	Group A. w = 139·776.	Group C. w = 199·674.	Group D. w = 259·500.
15·000	557·14	740·46	458·87	580·95	702·91
20	557·62	740·60	459·35	581·25	703·05
25	558·09	740·75	459·81	581·55	703·20
No. of col. }1	2	3	4	5	6

From this table we obtain the following results :—

Specific heat of water at 25° in terms of its specific heat at 15°, deduced from columns 4 and 6 .... = 0·99734

Ditto from columns 4 and 5.................... = 0·99722

Ditto from columns 5 and 6.................... = 0·99746

Mean .... = 0·99734

Hence, adopting 15° C. as the standard temperature, the

$$\text{SPECIFIC HEAT OF WATER} = 1 - 0.000266\,(t-15).^*$$

Also by means of equation (15) we get the following values of J : —

$$
\begin{aligned}
\text{Columns 4 and 6} & \quad\ldots\ldots\ldots\quad J = 4.1939 \times 10^7 \\
\text{,,}\quad\ 4\ \text{,,}\ 5 & \quad\ldots\ldots\ldots\quad J = 4.1940 \times 10^7 \\
\text{,,}\quad\ 5\ \text{,,}\ 6 & \quad\ldots\ldots\ldots\quad J = 4.1940 \times 10^7 \\
\hline
\text{Mean}\ldots\ldots\ldots & \quad J = 4.1940 \times 10^7
\end{aligned}
$$

This value of J, as previously pointed out (equation 5), is entirely *independent of the value assigned to the water equivalent of the calorimeter.*

And we find the water equivalent of the calorimeter at 15° C. in terms of water at 15° C. = 85.340 grams. The water equivalent of the calorimeter at 25° C. in terms of water at 15° C. = 86.174 grams.

Hence water equivalent $= 85.340\{1 + 0.000977(t-15)\}$.

We can now find the capacity for heat of the calorimeter and contents for any weight of water at 15°, 20°, and 25° C., and deduce the value of J from each group separately. The Groups B and E are experiments performed on 188.065 and 277.931 grams† respectively.

Table XLIII.—Values of J.

Group.	15°	20°	25°	Mean.
A	4.1940 × 10⁷	4.1940 × 10⁷	4.1939 × 10⁷	4.1940
B	4.1930 ,,	4.1941 ,,	4.1949 ,,	4.1940
C	4.1939 ,,	4.1938 ,,	4.1937 ,,	3.1938
D	4.1940 ,,	4.1939 ,,	4.1940 ,,	4.1940
E	4.1938 ,,	4.1940 ,,	4.1943 ,,	4.1940
				4.1940

We have in the above table given the values resulting from the calculation at different temperatures, for the limit of our experimental errors is thus clearly indicated, since the values of J ought (in the absence of experimental errors) to be identical at all temperatures. The close agreement between the values from different groups, and from the same group at different temperatures, is a satisfactory proof of the accuracy of our determination of the water

* Over the range 14° to 26° C.
† All weights are reduced to *vacuo*.

equivalents of the calorimeter, and of the changes in it and in the capacity for heat of the water. If we reject Group B (and we have already stated that it has little value), the results are practically identical.

Hence (the thermometry depending on comparisons with platinum thermometers) if we assume

1. The unit of resistance as defined in the 'B.A. Report,' 1892;
2. That the E.M.F. of the Cavendish Standard Clark cell at 15° C.
   . = 1·4342 volts;[*]
3. That the thermal unit = quantity of heat required to raise 1 gram of water through 1° C. at 15° C.,

the most probable value of

$$J = 4{\cdot}1940 \times 10^{7}.[\dagger]$$

This, by reduction, gives the following:—

$J = 427{\cdot}45$ kilogramme-metres in latitude of Greenwich ($g = 981{\cdot}17$).

$J = 1402{\cdot}2$ ft.-lbs. per thermal unit C. in latitude of Greenwich ($g = 32{\cdot}195$).

$J = 778{\cdot}99$ ft.-lbs. per thermal unit F. in latitude of Greenwich ($g = 32{\cdot}195$).

The length of this abstract is already unduly great, and we will, therefore, not enter on any discussion of the results beyond remarking that if we express Rowland's value of J in terms of our thermal unit we exceed his value by 1 part in 930, and we exceed the mean of Joule's determination by 1 part in 350.[‡]

The difference between Rowland's value of the temperature coefficient of the specific heat of water and ours would, however, cause both his and our values of J to be identical if expressed in terms of a thermal unit at 11·5° C.

---

* If we assume the E.M.F. of our Clark cells to be the same as that of the Cavendish standard (and we are inclined to think we have over-estimated the difference), we get $J = 4{\cdot}1930 \times 10^{7}$.

† The value obtained by us in 1891 = $(4{\cdot}192+) \times 10^{7}$.

‡ Rowland obtained the mean value of Joule's determinations by assigning values to different experiments, and the above comparison refers to the numbers thus obtained. If, however, we attach equal weight to all Joule's results, as given by Rowland, the mean *exceeds* our value by 1 in 4280, assuming our expression for the temperature coefficient of the specific heat of water.

III. "Studies in the Morphology of Spore-producing Members. Preliminary Statement on the Equisetaceæ and Psilotaceæ." By F. O. BOWER, D.Sc., F.R.S., Regius Professor of Botany in the University of Glasgow. Received January 30, 1893.

Still maintaining the same general views as were put forward in my preliminary statement on the Lycopodinæ and Ophioglossaceæ ('Roy. Soc. Proc.,' vol. 50, p. 265), I have now investigated other types from among the Vascular Cryptogams as regards the development of their spore-producing members. As some considerable time must still elapse before these and other results can be laid in full before the Society, a further preliminary statement will now be made of the more important facts recently obtained. It is assumed that readers will bear in mind the views put forward in the paper above quoted, as regards sterilisation of potential spore-producing tissue, the possible partitioning of an originally continuous sporogenous mass by bands of sterile tissue, and as regards the elaboration of external form which may follow on such partitioning. This will be specially necessary for the appreciation of the facts relating to the Psilotaceæ.

Taking first the Equisetaceæ, the development of the sporangia has been closely followed by Goebel ('Bot. Zeit.,' 1880–81); I find it, however, difficult to accept his conclusions as to the hypodermal origin of the archesporium.

On following the early phases of development in *Eq. arvense*, the sporangium is found to be eusporangiate, but the essential parts of the sporangium may be traced in origin to a single superficial cell, the cells adjoining this laterally contributing only to form the lateral portions of the wall. The first division of this cell is periclinal: *the inner resulting cell forms only a part of the sporogenous tissue;* the outer cell undergoes further segmentation, first by anticlinal, then by periclinal, walls, and *the inner cells thus produced are added to the sporogenous tissue, and take part in spore-formation.* The archesporium of *Eq. arvense* is thus shown to be not of hypodermal origin in the strict sense; the same appears to be the case in *Eq. limosum.* Similar additions to the sporogenous tissue by early periclinal division of superficial cells is commonly to be seen in *Isoetes*, and occasional cases, which are difficult to explain in any other way, have been observed in some species of *Lycopodium.* It would thus appear that Goebel's generalisation, that in all the Vascular Cryptogams which he investigated a hypodermal archesporium exists ('Bot. Zeit.,' 1880, p. 569), cannot be retained in the strict sense.

The tapetum is derived from the series of cells immediately sur-

rounding the sporogenous mass; it is, however, to be carefully distinguished from certain cells of the sporogenous mass, which also undergo an early disorganisation; for about one-third of the cells of the sporogenous mass do not form spores, but serve physiologically as a diffused tapetum, yielding up their substance to nourish the other young developing spores. This is another form in which sterilisation of sporogenous tissue may appear. A similar arrest of some of the sporogenous cells is found also in the Psilotaceæ.

I have already suggested a theory (*loc. cit.*, p. 273) of the mode of origin of the whole strobilus of *Equisetum* from a sporogonial head, and have no reason to alter my opinion on this point.

The synangia of the Psilotaceæ have given rise to voluminous discussions; one view, which is now very widely adopted, as to the morphology of the parts which bear the synangia of these plants is, that the synangium is terminal on an abbreviated axis, which bears in addition two foliage leaves. It will be seen that the investigation of the internal details of development will support a simpler and more probable explanation of the nature of these peculiar parts, that which was indeed generally held by the older botanists, viz., that the whole of each lateral appendage (*sporangiophore*) which bears the synangium is a single leaf. This conclusion had already been arrived at by Graf Solms, after examination of the external form of the developing organs in *Psilotum*, for he found the synangium to arise below the apex of the whole lateral appendage. No sufficient examination has, however, yet been made of the internal details of the development of these parts in the Psilotaceæ, excepting a few observations by Goebel on *Psilotum* ('Bot. Zeit.,' 1881, p. 688), which were, however, incomplete, through insufficiency of material.

Tmesipteris being the genus with the simpler structure, it may be described first. In their earliest stages of development, as lateral outgrowths from the axis, the sporangiophores are not readily distinguishable from the foliage leaves in form or structure, while they occupy a similar position upon the axis. In either case, a prismatic or wedge-shaped cell occupies the apex, as seen in radial section, but the tissues of the whole leaf are not readily referable to the segmentation of a single initial. The first appearance of a synangium is as an upgrowth of superficial cells of the adaxial face of the sporangiophore, immediately below its apex; meanwhile the cells of the abaxial side also grow strongly, while the apex itself does not grow so rapidly; so that the organic apex is soon sunk in a groove between these stronger growths. The superficial cells which are to form the synangium undergo periclinal and anticlinal divisions, to form about four layers of cells; all the cells of this tissue are at first very similar to one another, but, later, two sporogenous masses become differentiated; they are not, however, clearly defined while young

from the sterile tissue which forms the partition of the synangium, or from the wall, while the cells which form the partition are similar in their origin to the sporogenous cells. From the arrangement of the cells of these sporogenous masses it seems not improbable that each mass may be referable in origin to a single cell, but this has not been proved to be constantly the case. All the cells of the sporogenous tissue do not arrive at maturity, but here, as in *Equisetum*, a considerable number, serving as a diffused tapetum, become disorganised without forming spores. There is no clearly-defined tapetum in *Tmesipteris*. The leaf lobes begin to be formed almost simultaneously with the synangium, and appear as lateral growths immediately below the apex of the sporangiophore; their further development presents no characters of special note.

The synangium of *Psilotum* originates in essentially a similar manner, being formed from the upper surface of the sporangiophore, immediately below its apex. The details are different in accordance with the trilocular character of the synangium : but, as regards the structure of the wall and septa, the absence of any strict external limit of the sporogenous masses, and of any definite tapetum, as well as in the fact that a considerable proportion of cells of the sporogenous masses become disorganised without forming spores, *Psilotum* corresponds to *Tmesipteris*. Each sporogenous mass appears to be referable in origin to a single archesporial cell.

On the ground of the observations of internal development, of which the above are the essential features, I agree with the conclusion of Solms that the whole sporangiophore is of foliar nature, and that the synangium is a growth from its upper surface. The presence of the two lateral leaf-lobes need be no obstacle to this conclusion, while they serve an obvious purpose in protecting the synangium when young, more completely than a leaf of the form of the sterile leaves of these plants could possibly do.

For purposes of comparison with allied forms, *Tmesipteris* should be taken first : and the correspondence is most close with *Lepidodendron*, a fact which has a special interest since this genus and the Psilotaceæ are both very ancient types. In *Lepidodendron* the sporangium is very large ; it is narrow and elongated in a radial direction, extending a considerable distance along the upper surface of the leaf. I have already communicated to the Society the fact that trabeculæ extend in *Lepidodendron* from the base of the sporangium far up into the mass of spores (*loc. cit.*, p. 272), and have compared these with the trabeculæ in the sporangium of *Isoetes*. Neither of these sporangia are, however, completely partitioned. I now suggest that comparatively slight modification of the condition in *Lepidodendron* would produce the state of things seen in *Tmesipteris* : if the sterile trabeculæ of *Lepidodendron* were consolidated into a

transverse septum, and the apical growth of the sporophyll arrested and taken up by two lateral lobes, the result would be such as is seen in *Tmesipteris.* This is not a mere imaginative suggestion : it proceeds from the observed fact that the septum in *Tmesipteris* is undistinguishable at first from the sporogenous masses : here, as in other cases, it seems to me probable that a partial sterilisation of a potential archesporium has resulted in a partitioned sporangium. *Psilotum* itself illustrates, in its abnormal forms, the possible progression from two to four or five loculi. It may further be noted, in connexion with the above comparison between *Lepidodendron* and *Tmesipteris,* that the vascular tissues of some of the former appear to correspond more closely to those of *Tmesipteris* than to any other living plant.

Looking at the whole plants of the Psilotaceæ from the point of view above indicated, they are to be regarded as lax strobili, bearing sporangiophores (sporophylls) of rather complex structure. Branching, which is rare in *Tmesipteris,* is common in *Psilotum,* and is to be compared with the branching of the strobilus in many species of *Lycopodium.* In both there are irregularly alternating sterile and fertile zones, not unlike those of some species of *Lycopodium (loc. cit.,* p. 270) ; at the limits of these, arrested sporangia are frequently found. It is not difficult to imagine how such plants as the Psilotaceæ may have originated from some strobiloid type, not unlike that of the genus *Lycopodium.*

Those who accept the above suggestion will be prepared further to admit the comparison of the synangium of the Psilotaceæ with the " fertile frond " of the Ophioglossaceæ, which has been made by various other writers—Mettenius, Prantl, Strasburger, Celakovsky. This I believe to be a true homology ; I should, however, add to this the hypothesis that, in either case, we see the result of elaboration in size and form, together with partitioning, of a sporangium of an originally simple Lycopodinous type. In the Psilotaceæ the result may be 2—5 loculi ; in the Ophioglossaceæ the number may be in the simplest cases as low as six, or may rise to many hundreds in the larger species of *Ophioglossum* or *Botrychium.* It may further be remarked that every fresh case such as the above, where the development supports the hypothesis of partial sterilisation of a potential archesporium, and resulting partition, as exemplified among these lowest vascular plants, is of importance. A large body of evidence, to which I am now adding by the investigation of the Marattiaceæ, leads me to conclude that such partitioning of originally simpler sporangia has played a very important part in the evolution of vascular plants.

IV. " Further Experiments on the Action of Light on *Bacillus anthracis*." By H. MARSHALL WARD, D.Sc., F.R.S., Professor of Botany, Royal Indian Engineering College. Received February 13, 1893.

On December 15, 1892, I read to the Society a short account of my experiments[*] proving that the light of a winter sun and that of the electric arc rapidly destroy the life of the spores of the anthrax bacillus, and showed that the bactericidal action is really direct, and not due to elevation of temperature or to any indirect poisoning or starving process incident on changes in the food materials. I also mentioned that the evidence goes to prove that the effect is chiefly if not entirely due to the rays of higher refrangibility in the blue-violet of the spectrum.

The experiments have been continued with special reference to these latter points, and confirm the general conclusions in every detail. Not only so, but the further results prove that the inhibitory and deadly effects of direct insolation are not confined to *Bacillus anthracis*, but also extend to other bacteria and even to the fungi; and throw some light on several problems which have presented themselves during previous investigations.

Experiments with Screens of Coloured Glass.

I will first describe some experiments made during December to February with coloured screens of various kinds; premising that the methods employed in preparing and exposing the plates, &c., have been the same as those referred to in the previous communication.

Ten pieces of glass of various colours were selected, and each carefully examined as to thickness, transparency, and spectroscopic properties, and labelled so that all notes could be readily compared and contrasted.

Perhaps the best method of presenting the comparative peculiarities of these glasses is that employed in the following table (p. 24).

These pieces of glass were all of a size convenient for clamping over the outside of the stencil plate put on to the Petri's dish, and in which the letter or figure is cut (see p. 394 of previous paper).

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[*] See 'Roy. Soc. Proc.,' vol. 52, No. 318, p. 393.

Table I.

Number and colour of glass.	Thickness. in.	Transparency in contact.	Transparency at 45°.	General spectroscopic characters.	Absorption bands.	Other peculiarities.	Remarks.
Blue 1......	¹⁄₁₀	Excellent	Excellent	Much of all rays pass	Marked bands in red and yellow	Green very bright at less refractive half; other half weak	Superposed, these cut off ⅔ more refrangible red, all orange-yellow, and ⅔ more refrangible green
Blue 7......	¹⁄₁₆	Just read print	Very good	All but yellow-orange through	Broad band blocks off yellow-orange; strong band in red	More refrangible green weakened; blue-violet unaltered	
Violet 2 ..	{⅛ ¹⁄₁₆}	Invisible	Fair only	Some of all rays pass?	Band in yellow, and some blue absorbed	Chiefly red, green, and violet transmitted	Glass a long oblong, ⅛-inch thick at one end, thinning off to ¹⁄₁₆th at the other. Superposed with blue 1, great loss of blue
Ruby 3.....		Excellent	Excellent	Cuts off all yellow and green to violet	.....	Only red and yellow pass	These two superposed, same as ruby 5 alone
Ruby 5.....		Invisible	Scarcely see to read	Cuts off all beyond first ⅔ of red	.....	Only lets first ⅔ of red through	
Olive 4.....		Almost perfect	Almost perfect	All to green pass, and a trace of blue-green	.....	All violet and blue (or nearly so) cut out	
Orange 6....		Excellent	Excellent	Only red, orange-yellow, and ⅔ green through	.....	Cuts off all beyond first half of green	..
Green 8....		Just seen by day	Very good	All but red and indigo-violet pass to some extent, but especially green	.....	Cuts out most of orange-red and blue-violet, but a little less refrangible blue passes	..
Ordinary 9. Ordinary 10		Perfect Perfect	Perfect Perfect	All pass All pass	.....	.....	.....

Table II (p. 26) summarises the results of a series of experiments made with these glass screens, and shows at a glance all the essential points brought out in the experiments.

The first column merely gives the number of the experiment. Column 2 is occupied with the tabular number of the particular glass screen employed, and is more fully elucidated by reference to the properties summarised in Table I. In column 3 we have the kind of plate employed. In all cases the ripe spores were used, and it only remains to add that it makes no essential difference to the result whether agar, or gelatine, or a mixture of both, supplies the food material, except that agar is, of course, much the most convenient medium for incubation. With regard to Experiments 11 and 12 in this table, the backs of the lids of the Petri's dishes were slightly blackened in the smoke of a candle, in order to reduce the internal reflection, and I ought to state that numerous trials have shown that this does not seriously affect the results.

It has the disadvantage, however, of sometimes causing black spots on the plates, owing to the condensed water drops carrying off some of the soot; of course this spoils the neatness of the cultures, and, I confess, I was surprised that nothing worse happened in spite of the slight acidity of these drops.

Columns 4 and 5 simply give the dates of making and exposing the plates. Data are given in the column of remarks, from which the reader can judge of the hardships to which the plates are often exposed in the intervals—during sunless days and at night—and these will be referred to later on.

In column 6 we see how long each plate was exposed to the direct rays of the clear sun, and it is only necessary to add that four hours of really bright sunlight is usually quite sufficient; indeed, as reference to the previous communication shows (p. 394), excellent results have been got with exposures of two hours to a December sun.

In column 7 are given the number of days that the plates were incubated at 20° C. after exposure. In those cases where a positive result was obtained, the time when the letter was distinctly visible is recorded: where no letter, and therefore no bactericidal action, was observed, the plates were incubated until the growth of the anthrax was thick and decisive.

Then comes the column summarising the results, and showing at the same time what letter was employed. It only remains to point out that no bactericidal action was detected behind the screens of experiments No. 2 (olive), No. 4 (ruby), No. 6 (ruby), No. 7 (olive), No. 9 (orange), No. 11 (ruby), and No. 12 (green), and if we compare the data in Table I, this can be put as follows :—

There is no perceptible bactericidal action behind any of the glass

Table II.

Number of experiments.	Nature of screen.	Kind of plate.	Date made.	Date exposed.	Hours of sunshine.	Days incubated.	Result.	Remarks.
1	Violet 2	Spores and gel. agar	Dec. 25	Dec. 25 and 26	9	1	Distinct, though blurred M, owing to softening of gelatine	
2	Olive 4	,,	,,	,,	,,	4	No trace of E	Exposed 4½ hours each day. The failures were incubated as long as possible
3	Blue 1	,,	,,	,,	,,	1	Sharp H	
4	Ruby 8	,,	,,	,,	,,	4	No trace of X	
5	Ordinary 10	Spores agar	Dec. 31	Jan. 4	4	3	Good O	In dark at 12° C., Dec. 31—Jan. 4, and germinated. Jan. 4—6, 0—2° C. in and out
6	Ruby 8	,,	,,	,,	,,	5	No letter E	
7	Olive 4	,,	,,	,,	,,	5	No letter X	
8	{ Blue 1* Blue 1* Blue 7 }	Spores agar	Dec. 31	{ Jan. 10 and 11 }	6	3	Good H	No. 8, only two blues over one half. All at 12° in laboratory from Dec. 31 to Jan. 10. Then 2°—5° in and out. Exposed 1 hour Jan. 10, and 5 hours on 11th
9	Orange 6	,,	,,	,,	,,	7	No trace of H	
10	Ordinary 9	,,	,,	,,	,,	7	Dried up without germination at first; good M eventually	
11	Ruby 5	Spores agar, smoked back	Jan. 8	Jan. 10 and 11	6	7	No trace of E	Over 9th, at 10° C. Other intervals, 2·5—5° C.
12	Green 8	,,	,,	,,	,,	7	No trace of X	
13	Violet 2	Spores agar, smoked agar back	,,	,,	,,	2	Sharp E	
14	Olive 4	,,	,,	,,	,,	7	No trace of O	

No.	Screen	Medium	Date	Dates			Description	Remarks
15	Half blue 1; half violet 2	Spores agar	Jan. 27	Jan. 27 to Feb. 1	6	2	Area under blue quite clear; that under violet has some colonies	2 hours' sunshine on Jan. 27, and 4 hours' on Feb. 1. Interval dull. Temperature averaged 10—13° C.
16	Green 8	"	"	"	6	10	Traces of inhibition in centre of plate, but no letter	⎫ Exactly same conditions as No. 15
17	Half ordinary 10; half orange 6	"	"	"	6	2	Area under ordinary glass quite clear; orange showed a faint trace of effect on 2nd day, but grown over on 3rd	⎬
II	No screen	"	"	"	6	2	Good figure 4 (in duplicate)	⎭ An interesting result was obtained. On the 27th, after 2 hours' exposure, I inadvertently shifted the stencil plate, and fixed it anew; *two imprints* were visible on the 3rd, a less sharp one corresponding to the 2 hours' sun of Jan. 27
19	Ordinary 9	Spores agar	Feb. 1	Feb. 1 to 4 inclusive	7½	■	Excellent U	Germination centripetal, and U very faint at first, then sharp
20	Green 8	"	"	"	7½	5	No letter	There was some evidence of slowing, however
21	Half under superposed blue 1 and violet 2; half under green 8	"	Feb. 4	Feb. 4 to 6 inclusive	12	::	::	See Table I, *re* violet 2

screens which transmit the red, orange, and yellow, but cut off the blue and violet, rays.

The green screen (No. 12) will have to be considered separately, and remains doubtful for the present.

On the other hand, distinct positive results were obtained in the following experiments:—No. 1 (violet), No. 3 (blue), No. 5 (ordinary), No. 8 (blue), and No. 10 (ordinary) ; in other words—

The bactericidal action is marked behind the glass screens which transmit most or all of the blue and violet rays, whether they cut off the red, orange, and yellow rays or not.

These results may be expressed still more shortly by saying that the bactericidal action of the sun's rays is due to those in the blue-violet half of the spectrum, a result perfectly in accordance with the evidence obtained by working with the spectrum thrown on the plates.

As is well known, however, it is possible to test such a generalisation by using screens of other kinds, and my experiments with these are interesting, as showing not only the accuracy of the above conclusions, but also that the light is effective after passing through considerable thicknesses of water.

Experiments with Screens of Chemical Solutions.[*]

I employed saturated solutions of *ammoniated cupric oxide* and of *potassium bichromate:* as is well known, the first cuts off all the red, orange, and yellow rays as far as the line *b* (in the green), while the second cuts off the blue-violet from the line *b* onwards.

On January 10, I made two plates of spores in agar, poured from the same tube, and alike in all respects : they remained at 8—10° C. till the 12th, when each was exposed. One was covered with a stencil in which the letter T was cut, put behind a screen of *ammoniated cupric oxide*, the glass faces of which measured each $\frac{3}{3\cdot5}$ inch, and the solution $\frac{7}{16}$ inch ; the other, with an exactly similar letter T, behind a screen of *potassium bichromate*, measuring $\frac{1}{16}$ inch in thickness of each glass, and $\frac{9}{3\cdot5}$ inch of the solution.

They were exposed to the south, side by side, on the 12th and 13th, both dull days, with a gleam of sunshine for about one hour and two hours respectively. A large concave mirror was also placed so as to throw more light on each, as equally as possible. The temperature rose to 20° C. occasionally, but not beyond. On the 14th they were exposed for three hours to dull diffuse daylight, there being no sunshine.

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[*] Such screens have been used, *e.g.*, by Janowski ("Zur Biologie der Typhus-bacillen," 'Centralbl. f. Bakt.,' vol. 8, 1890, p. 167), but both his methods and results were different. It is inferred that the *red* rays are active, but his screen (a solution of fuchsin) probably allowed transmission of violet rays, as I have convinced myself by spectroscopical examination.

On January 16, the plate behind the thick blue screen showed an extremely faint letter; that behind the thinner and much more transparent orange screen, a letter, perhaps as distinct, but also extremely faint. The inhibition here was very slight indeed, and the effects were obliterated after a few hours' further incubation; moreover, I found that with a stronger solution of the potassium bichromate than I could use in the very cold weather, the inhibiting rays are kept out altogether.

On January 14 an agar plate of anthrax spores, made the previous day and kept for twenty hours at 5—7° C., was exposed for five hours to brilliant sunlight behind a thick screen of *ammoniated cupric oxide*; the light had to traverse the two glass faces of the screen (each $\frac{1}{16}$ inch), and that of the plate—the bottom of a Petri's dish—as well as the solution, which was $\frac{7}{16}$ inch thick. When held up to the sun, the latter was just plainly visible through the deep violet blue solution; but the letter T of the stencil plate covering the culture was perfectly invisible.

On the same date, and exposed for the same time, side by side with the above culture, an exactly similar plate poured from the same tube, &c., was put behind a screen of *potassium bichromate*, which was so transparent that the letter (also a T) of the stencil plate was distinctly visible through it. Moreover, this screen was only $\frac{9}{32}$ inch thick, and each glass face $\frac{1}{16}$ inch.

On January 16, *i.e.*, after little more than twenty-four hours' incubation, the plate from behind the blue screen, showed a perfectly sharp letter T, whereas there was no trace as yet on that from behind the orange screen; the latter plate, however, showed an extremely faint letter after two more days' incubation, proving that the rays transmitted through the *potassium bichromate* solution were slightly inhibitory, though very much less so than those passing the *ammoniated cupric oxide*.

Further experiments, in which the two screens were interchanged, only confirmed these results, and carried them a step further, showing that the light passing through the *blue* solution, whether in the thick or the thin screen, always gives a sharp and clear letter, whereas that through the orange solution, exposed side by side, and for the same time, is always very faint, if it comes out at all, and it only appeared in my earlier experiments, when, owing to the low temperatures, the bichromate solution was too dilute. Not only so, but the very dim *blue* light (as estimated by the eye) which traverses the *thick* screen of copper salt is far more bactericidal than the much brighter *orange* light transmitted through the *thinner* screen. In other words, the more refrangible *blue* rays are the effective ones in destroying the life of the spores.

Experiments with Spores and Food Material on Separate Plates.

In order to test still further the accuracy of my previous conclusions, that the bactericidal action of the sunlight is direct, and not due to secondary effects, owing to changes in the food material, I carried out the following modifications of the experiments.

Agar tubes of anthrax, cultivated for several days at 25° C., were selected and examined for spores; those were selected which exhibited a rich crop of thoroughly mature spores. As is well known, it is possible to remove from such cultures the thick, yellowish, creamy layer of spore material without taking any appreciable quantity of the agar, and great care was employed to accomplish this successfully. Having shaken up these spores in sterile distilled water in a test-tube, I poured a few drops of the infected liquid into each of several Petri's dishes at 70° C., in a hot-air oven at that temperature, and replaced the dishes to allow the water to evaporate into the hot air during the cooling down of the whole. After two or three hours the water had evaporated completely; or, if not, the oven was heated up again to 70°, and the drying completed, leaving a thin film of dry spores sticking to the bottom of the glass dish. All bacilli and immature spores were of course killed by the high temperature, and only the thoroughly ripe and resistant spores left alive.

These plates were then wrapped up with suitable stencil plates, &c., exactly as in my previous experiments, and exposed to sunlight, as before described.

For each plate of spores alone thus prepared, I also made a corresponding plate of agar alone, and exposed these side by side with the plates of spores.

Obviously, if the bactericidal action was *direct*, it ought to be manifested on the cultures of spores alone, if those spores not killed by the light could be made to develop in a thin layer of fresh agar poured on the plates after exposure; whereas, if *indirect*, and due to changes in the agar, the effect ought to be visible by spreading a thin layer of fresh spores over the agar which had been insolated.

If both effects occur, then both sets of plates should show the positive results.

Of course this was a case where negative results with *both* plates could not be used for any useful conclusions; for it is quite conceivable that the destructive light-action might not occur in *dry* spores, or that some volatile body* might be formed in the agar, which was rapidly dissipated in the water used for the sowing of fresh spores after insolation; or again, the agglutinated layers of dry spores might shelter one another from the sun's rays, and so inter-

* *E.g.*, it is not inconceivable that hydrogen peroxide might be formed from the moisture in the agar.

fere with the results obtainable when the spores are distributed through a matrix of agar. Finally, it was possible that the aërobian dry spores would not germinate with a layer of agar *poured over them*, although I employed the thinnest coverings possible.

On the other hand, positive results with the dried spores would be conclusive as to the main point at issue; the actual results seem to me to set the matter at rest, for they prove that the action of the solar rays is *direct on the ripe spores*, and that the food material of the culture media is not appreciably concerned at all (see Table III).

Passing over a number of experiments where gelatine was employed, and which were rejected because this medium is too soft and runs too easily to yield good prints, I select the following:—

On January 20 two plates of plain agar were made as thin as possible; one (Table III, A) had a stencil C, the other (Table III, B) an H, for its letter. On January 22 two plates were also made as follows:—Two Petri's dishes were heated to 130° C. in the hot-air oven: when the temperature of the oven had fallen to 90° C., each plate received a few drops of sterile distilled water strongly infected with anthrax spores, and the fluid was distributed as evenly as possible over the base of the hot dishes. The whole was then left to cool, when each dish had a thin film of the dry spores sticking to it. One (Table III, C) was covered with a stencil letter H, the other (Table III, D) with a small B.

All four plates were exposed daily till January 27, but the weather was so dull that they received practically no sun on the 20th to the 26th inclusive: the 27th was very bright, however, and each plate had at least five hours' good insolation that day. At sundown I distributed spores in water on the two pure gelatine plates, and a thin layer of gelatine over the two plates of pure spores.

On the 29th the letter H was clearly visible on the one plate of the dried spores (Table III, C), and the B faintly marked and blurred on the other (Table III, D), proving that spores had been killed over the area—H and B—exposed to light, but not elsewhere; whereas the two plates made with agar only were uniformly covered with the anthrax growth, showing that the spores had germinated equally all over, and therefore that the agar exposed to the sun (on the areas C and H) was not rendered incapable of supporting their growth.

On January 27 I made two thin plates of agar only, without spores, and two plates of spores only, dried at 70° C., as described above.

Two of these plates (Table III, E and G) were exposed forthwith at 1 P.M., and received over two hours' direct sunlight. Each was then—about 3.30 P.M.—put in the incubator, after receiving a charge of gelatine (on the spores) and of spores in water (on the agar) respectively.

Table III.

Number of plate.	Kind of plate.	Date made.	Dates exposed.	Hours of sunlight.	Days incubated.	Results.	Remarks.
A..........	Agar only	Jan. 20	Jan. 20 to 27 inclusive	5	2	Negative	The spores were germinating all over, and no results were obtained on keeping longer
B..........	Spores only dried at 90°—60° C.	Jan. 22	"	5	2		Gelatine was used to pour over the exposed spores, but it runs too much for good prints
C..........	"	"	Jan. 22 to 27 inclusive	5	2	Letter H distinct	
D..........	Agar only	Jan. 27	Jan. 27, 1 P.M.	5	2	Very faint letter B	It had 2 hours' good sun on Jan. 27 and 4 hours' Feb. 1; intervening days dull
E..........	"	"	"	2	3	Negative	
F..........	"	"	Jan. 27 to Feb. 1	6	4	"	
G..........	Spores only dried at 70° C.	"	Jan. 27, 1 P.M.	2	2	"	Used gelatine, and it ran badly on the 29th
H..........	"	"	Jan. 28 to Feb. 1	4	2	Sharp and distinct letter C	Agar was used
I..........	Agar only	Jan. 29	Jan. 31 and Feb. 1	4	3	Negative	The light was reflected from mirror below
J..........	"	"	Jan. 31 to Feb. 4 inclusive	9	..	..	It had 4 hours' good sun *direct* on Feb. 1, and 5 reflected from a mirror below on the 4th
K..........	"	"	Jan. 31 to Feb. 3	4	4	No trace of letter	Kept longer, but the germination proceeded quite evenly all over
L..........	Spores dried at 70°—60° C.	"	Jan. 31 to Feb. 4 inclusive	9	..	..	Exposed exactly as No. J, of which it is the reciprocal
M..........	"	"	Jan. 31 to Feb. 8	4	2	Faint, but distinct H	This is reciprocal to No. K, and treated exactly in same way
N..........	"	"	Jan. 31 to Feb. 1 inclusive	4	2	Perfectly sharp and clear +	Treated exactly as No. I, of which it is reciprocal
O..........	Spores only dried at 70° C.	Jan. 31	Feb. 1, 1 P.M., to Feb.	10	2	Letter Z coming up very slowly	The internal reflection had caused wide-spread inhibition around
P..........	"	"	Feb. 1, at 1 P.M., to	10	2	Excellent letter E	Similar inhibition observed around

On the 29th the spores had germinated all over on both plates, and the results were negative. This was probably in part due to the exposure being too short. Nothing was gained by keeping the agar plate another day.

Of the other two plates, the agar preparation (Table III, F) was exposed again daily till February 1 inclusive: this latter date was the first sunny day ensuing, the intervening ones being dull or wet. The other spore plate (Table III, H) was not put out till January 28, as it was not quite dry on the 27th; it received the same exposure otherwise.

On February 1 there were at least four hours' good bright sunshine—more probably, only I allowed for interruptions by clouds. Other details may be seen by referring to Table III, E to H.

On February 3 no trace of a letter could be detected on the agar plate (Table III, F), and the spores sown thereon were germinating evenly all over; whereas a perfectly sharp and distinct C was visible on the spore plate (Table III, H), although it had only been insolated for *four* hours instead of six.

It is not necessary to detail all the confirmatory experiments, and I will conclude with the following:—

On January 29 I made three plates of agar alone and three of spores (Table III, I to N), drying off the latter at 70° to 60° C. All were left till January 31, because the 30th was a very dull day. They were all put out on the 31st, but did not receive more than 15—30 minutes of interrupted sunshine at midday. February 1 was a bright day, and each plate had at least four hours' direct sun. One of each of the plates (Table III, I and N) was then incubated, after receiving its reciprocal charge—agar on the spores, spores on the agar. The others were left for further exposure.

On February 3 the agar plate showed no trace of its letter (a small B), whereas the spore plate showed a beautifully sharp +, the shape of the area exposed, and over which the *dry ripe* spores were killed. February 2 was a dull day, and the 3rd rainy, so that the remaining plates got no sun until February 4, which was a beautifully bright day, giving me at least five hours' clear sunshine.

Two more plates, however (*i.e.*, one of each kind, Table III, K and M), were put into the incubator at 8.30 A.M. on the 4th, and had therefore only received the four hours' real sunshine credited to the preceding plates, though of course they had been exposed to two more days' diffused daylight. See Table III. I have recently modified this procedure, however, to try to obviate the following difficulty.

I found that it is very difficult to pour the agar, &c., over the film of insolated spores without sweeping some of the non-insolated ones over the area left uncovered, and *vice versâ;* and, again, it is not

easy to wash a perfectly even layer of spores over the insolated agar or gelatine. Further, a critic might object that sowing spores on the *upper* surface only of the insolated agar might yield negative results, because the light-action does not extend through the entire thickness of the plate, or because some volatile body is formed which is washed off with the sowing.

My new procedure is as follows:—I make, for instance, two plates of dried spores only, and two of agar only, all as before. I then expose one plate of each kind, and keep the others in the dark.

After exposure I remove the stiff and moist film of *non-exposed* agar from its own plate, and *superpose it on the exposed film of dried spores in situ.* Reciprocally, I remove the film of *exposed* agar, and *superpose it on the non-exposed film of dried spores.*

This prevents any wash or displacement, and ensures at the same time that the agar shall present in contact with the spores that face which was next the source of light.

So far I have been unable to observe any appreciable effect on the agar, though the dried spores exposed for an equal period are killed in abundance. Whether exposure to more intense sunlight will make it necessary to modify this must await experiment.

Preliminary Results with the Spores of Fungi.

Results substantially the same as the above are obtainable with other *Schizomycetes*, but it was interesting to see whether anything of the kind occurs with the spores of true Fungi. The time of year has, for many reasons, been unfavourable for very numerous experiments, but the results so far are extremely encouraging, and should, I think, give a stimulus to close enquiry into the whole subject.

I have so far employed the following species:—*Penicillium crustaceum, Aspergillus glaucus, Botrytis cinerea, Chalara mycoderma, Oidium lactis, Nectria cinnabarina, Mucor racemosus, Saccharomyces pyriformis,* and a " *Stysanus* " conidial form met with some months ago as a saprophyte on *Pandanus.*

On making agar and gelatine plates of these as before, I have obtained positive results with *Oidium* (5 cases), *Chalara* (1 case), *Saccharomyces* (4 cases), *Stysanus* (2 cases), and negative results with *Aspergillus* (5 cases), *Penicillium* (2 cases), *Mucor* (2 cases), *Nectria* (4 cases), and *Botrytis* (2 cases).

It seems worth noting that, in all the forms which have given me a positive result right off, the spores, as seen in masses, are either hyaline and colourless, or, in the case of the *Stysanus,* with a faint tinge of buff; whereas those which gave negative results are either of some very pronounced colour, as *Aspergillus, Penicillium,* and *Nectria,* or (*Mucor* and *Botrytis*) of a dull, yellow-brown hue.

Theoretical.

It remains to be seen whether any explanation can be given that meets all the facts. The following suggestions seem fairly deducible so far.

The spore is, in its fully ripe condition, a remarkably passive body, and contains a smaller or larger store of reserve food materials, generally of an oily or fatty nature.

It is commonly conceded that the ripe spores of bacteria contain a highly refringent substance like oil, and we know from the almost universal occurrence of fat-like oils in the spores of Fungi and other plants, including pollen grains, that such bodies constitute a very common form of reserve material. Not only so, but the resistance of spores to ordinary processes of staining and of sterilisation is intelligible if they contain fatty substances. This, of course, does not exclude the importance of the membrane and the other contents of a spore in regard to its power of resistance.

It appears at least possible that the bactericidal action of the light is due to its destructive influence, in presence of oxygen, on the fatty matters or other oxidisable substances forming the reserve materials of the spore. Duclaux* has at any rate shown that vegetable oils, such as olive and palm oils, are rapidly oxidised if exposed to light, and, although we ought to be very careful in deducing any explanation of physiological phenomena directly from chemical experiments, we have here the significant fact that it is the rays known to be so commonly concerned in promoting chemical changes which are concerned in destroying the spores. The spores are, in their passive resting state, to a certain extent comparable to a flask of oil exposed to oxidation in the sunlight. Moreover, the bacilli, which contain little or no fat, and the protoplasm of which is far more accessible to the light, are found to resist the bactericidal action of the light to a much greater extent than the spores.

Of course, it may turn out that the action of the light is more profound than the simple explanation offered assumes, and that the physiological properties of the protoplasm are deeply concerned, but I cannot help thinking that if this were the case we ought to obtain more evidence of the action with the actively growing bacilli than seems to be forthcoming from the results of the experiments. In any case, it must be allowed that the solar action is direct on *something* which is readily oxidisable in the spore: if the action were on the protoplasm one would expect to find the vegetative bacilli succumb more easily than the spores.

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* Duclaux, 'Ann. Inst. Pasteur,' vol. 1, 1887, p. 353. Duclaux says the oxidation is the more rapid the more finely divided the oil is. See also Ballantyne, 'Journ. Soc. Chem. Ind.,' 1891, p. 29, and Hartley, 'Journ. Soc. Arts,' 1893, p. 286.

The assumption that the action is direct on some oxidisable body, like a fat, in the spore would help us to explain a good many facts, and among others the death of the protoplasm, if insolation is complete, because we know how intolerant of traces of acids *Bacillus anthracis* is, and inhibition and attenuation if the insolation is incomplete, and the protoplasm not so far injured that it cannot work up the remnants of reserve materials left.

That this point is one well worth further investigation is too obvious to dwell upon here, and I hope to succeed yet in discovering what is the body destroyed.

Literature.

There are, so far as I can discover, very few statements to hand as to the influence of light on spores other than bacteria,* and their germination; and, indeed, as shown by Elfving's excellent summary of the literature in his ' Studien über die Einwirkung des Lichtes auf die Pilze,' as to the influence of light on Fungi in general. It has been the custom to regard the effects of light on the Fungi as not essentially different from those on other plants; in accordance with De Bary's general statement to that end on pp. 352—353 of his ' Comp. Morphol. and Biol.,' Engl. Ed., 1887.

Zopf (' Die Pilze,' 1890, p. 199) believes that light, as a rule, exerts no influence whatever, either on the germination of the spores or on the development of the mycelium, though he himself quotes De Bary's and Wettstein's statements to the contrary, and I have myself observed similar cases.

De Bary (' Ann. des Sc. Nat.,' vol. 20, 1863, p. 40) found that the zoospores of certain Peronosporeæ are inhibited by light in their germination, *e.g.*, *P. macrocarpa* and *Phytophthora infestans*, a fact I should correlate with their delicate walls and want of protective colour screen. On the other hand, De Bary (' Ann. des Sc. Nat.,' IV Series, vol. 20, 1863, p. 54) showed experimentally that the pustules of *Uromyces appendiculatus* appear preferably on that side of the leaf which receives most light, and this is indicated by their occurrence on the upper faces of these organs, facts quite in accordance with the deep orange-red colour of this fungus.

In *Uromyces* and others of these orange Uredineæ, the colouring matter is associated with fatty drops (Bachmann, " Spectroskop. Unters. von Pilz-farbstoffen," in ' Progr. des Gymnas. zu Plauen,' Ostern, 1886, pp. 9, 21), and shows two absorption bands, one on the boundary between the green and the blue near F, the other in the blue between F and G, and the whole of the violet beyond G is cut

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* The literature, so far as bacteria are concerned, will be found in ' Roy. Soc. Proc.,' vol. 51, 1892, p. 237, and vol. 52, 1893, p. 393.

out. Other Uredineæ exhibit the same phenomena, as Bachmann and Zopf ('Die Pilze,' p. 145) showed.

In short, the whole series of red, orange, and yellow pigments of fungus spores and flowers are at least calculated to act as screens which cut off more or less of the blue end of the spectrum, and they agree remarkably throughout in this particular.

Hoffmann ('Pringsh. Jahrb.,' II, p. 321) observed that the spores of *Uredo destruens* germinated as well in light as in dark, whereas those of *Agaricus campestris*, in one case, germinated better in the dark than in the light—both cultures side by side.[*] Light did not interfere with the germination of *Fusarium heterosporium* (a red form on Rye), *Trichothecium roseum*, *Penicillium glaucum*, *Uredo destruens*, and *U. segetum*. Here, again, we have the red-yellow pigments predominating, and, I suggest, acting as screens. With regard to *Penicillium*, it must by no means be concluded that the peculiar bluish-green hue gives us any information as to the rays transmitted to the interior of the conidium: the spores of *Aspergillus* appear yellowish by transmitted light, though, like *Penicillium*, they are blue-green by reflected light, when seen in large masses.

Löw ('Verhandl. d. Zool.-bot. Gesellsch. in Wien,' 1867) also finds no effect of light on *Penicillium* and *Mucor stolonifer*: so far as *Penicillium* is concerned, this confirms Hoffmann, and my own attempts with it and with *Aspergillus glaucus* are in accordance.

Wettstein ('Sitzungsb. d. Wiener Akad.,' 1885, B. 41, p. 39) found that light inhibits the germination of the very delicate conidia of *Rhodomyces Kochii*, a parasite in the human alimentary canal.

As regards my own older experiences with germinating spores, I have a vivid recollection of the great difficulties experienced in 1886 with the conidia of *Phytophthora infestans*, and although the meaning of the facts observed was not then clear to me, reference to my paper on this fungus in the 'Quart. Journ. Micr. Sc.,' vol. 27, N.S., 1887, p. 422, proves that the idea of increased oxidation under the influence of intense light had already suggested itself to me.

During my investigations into the biology of the orange-yellow Uredine, *Hemileia vastatrix*, the fungus of the Coffee-leaf disease, on the other hand, the impression is equally clear that little or no difficulty occurred with germinations of any of these orange-coloured spores, even in the intense tropical light of Ceylon.

Many similar, but less vivid, impressions of the connexion between light and germination occur to me, but little can be said as to their value now.

L. Klein ('Bot. Zeit.,' 1885, No. 1, p. 6) found that, as Rindfleisch ('Virchow's Arch. f. path. Anat.,' B. 54) had already observed, the

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[*] At the same time, there is possibly a slight confusion here, for De Bary states (p. 352) that the germination of *A. campestris* has never been certainly observed.

conidia of *Botrytis cinerea* will not develop in daylight. If the conidial branches are not completed before sunrise, the whole process stops till the darkness comes on again, and this, no matter when the sowings are made. Klein also proved that this inhibition is due to the blue-violet rays, and is in abeyance in proportion as they are removed.

Here, then, we have the light action effective in preventing the storage of the spore materials, just as it is in germination in preventing their return into the area of metabolic activity.

Sorokin found ('Botan. Jahresbericht,' 1874, p. 214) that if fresh horse-dung is placed under screens of white glass, bichromate of potassium, and ammoniated cupric oxide, in the light, for twenty-five days, and compared with one another and with specimens in the dark, the feeblest crops of fungi were obtained in the *blue* light.

Laurent ('Comptes Rend. Soc. roy. de Bot. de Belgique,' T. 28 (2), 1889, p. 162) exposed spores of *Ustilago carbo* in glass flasks to direct sunlight, in July, and found eight hours' insolation killed them ; whereas the controls were all right, and sixteen hours' insolation behind a screen of quinine sulphate, cutting off the ultra-violet, did not prevent their germination. The spores of *Aspergillus niger*, *A. glaucus*, *Botrytis cinerea*, and *Cladosporium* proved much more resistant. The spores of *Penicillium* were also found to succumb to insolation.

Elfving found ('Studien über die Einw. d. Lichtes auf die Pilze,' 1890, p. 105) that intense sunlight in summer inhibited the germination of *Aspergillus*, but even several days or weeks of insolation did not kill the fully ripe spores. If they had begun to germinate, however, the first germinal stages were very sensitive to sunlight; and they were equally sensitive when just developed, *i.e.*, just before ripening.

The whole tenor of Elfving's work, which is more particularly concerned with the vegetative mycelia and the weight of substance developed, is to show that *light inhibits the metabolic processes the younger the tissues are, and the more the absorbed food materials approximate to the raw state.* This is not contradictory of my idea that the light destroys such substances as oily reserve materials directly in presence of oxygen, and there is nothing surprising in the action being facilitated on the germination of the spore—no doubt the most dangerous moment is that when the material begins to pass into the area of metabolic activity in the germinal hypha.

The consideration of the above and other facts led me to formulate to myself the following hypothesis :—

No plant exposes a reserve store of fatty food materials to the danger of prolonged or intense insolation without a protective colour screen, calculated to cut out at least the blue-violet rays, as these rays would

*otherwise destroy the reserve substance by promoting its rapid oxida-
tion.*

Colours of Fungi.

One of the most interesting cases for our purpose is the colours of the spores of Fungi.[*]   I find very little information as to the correlation between the habits of Fungi and the colour of their spores ; but a somewhat hurried examination of the matter has yielded the following information :—

Taking the higher *Hymenomycetes* first, since more attention has been paid to the colour of their spores, as is well known, the large genus of *Agaricus* is usually broken up into five groups, according to the prevailing colour of the basidio-spores.   I confine myself to the British species.

Of the white-spored Agarics (*Leucospori*) I find that Stevenson[+] mentions about 400, and of these the vast majority are found in woods, and many of those occurring in pastures seem to me to be probably well sheltered by the herbage.   Cook[‡] notices that they are in contrast with the dark-spored groups in not growing on dung or in rank places.

As regards the next set, the *Hyporhodii* or pink-spored forms, it appears not without significance that, while some grow in open woods, about two-thirds are found in more exposed situations.   Mr. Plowright was under the impression that the spores of these *Hyporhodii* germinate easily ; if this is so the fact is significant.

The *Dermini*, with yellowish to clay-coloured spores, present mixed features ; it seems to me that not quite half grow in more or less woodland places, often very open, while a little more than half grow in more exposed situations.   It must be noted, however, that the colours of the spores in this group vary more as to shade than in any of the others. ·

The fourth group of Agarics are the *Pratelli*, with spores of very various shades of purple tinged with black or with fuscous hues, and passing into brown, violet, slaty, and even pink hues.   The majority by far grow in exposed places.

There is a curious remark in Stevenson, regarding these *Pratelli :* . "It is to be observed that the spores vary in colour according to the colour of the ground on which they are deposited." This seems clearly to indicate that these spores respond to the kind of light they receive, and seems worth pursuing further.

Lastly, we have the black-spored *Coprinarii*, none of which are

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[*] Elfving (*loc. cit.*, p. 53) suggests that the coloured pileus of some Hymenomycetes may act as colour-screens, and the matter wants investigation.

[+] 'Hymenomycetes Britannici,' 1886.

[‡] 'Handbook of British Fungi,' vol. 1, p. 5.

typically wood-fungi, and nearly all of which grow in open exposed situations, on dung, &c.

If I sum up the foregoing remarks about the genus *Agaricus*, it amounts to this :   There are nearly 800 British species described, and of these 400 or so are white-spored, and nearly all grow in the shade of woods, the remainder chiefly in the long grass of meadows, &c., the remaining moiety, a little short of 400 more, have spores of various colours (pink, yellow, brown, purplish, and black, &c.), and grow for the most part in places more exposed to the light.

Generally speaking, the same seems to hold good for the other genera of Agaricini.   *Coprinus* has black spores, and grows in exposed places on dung, and similarly with the rusty-spored *Bolbitius*. *Lactarius, Russula*, and *Cantharellus* have white to yellowish spores, and grow in woods.

At the same time there are difficulties.   The genus *Cortinarius* is a large one, of about 130 species, with yellowish spores, and all, or nearly all, grow in woods ; whereas *Hygrophorus*, a white-spored genus, comprising a little over fifty species, has half of them growing in " grassy places."   It may be that a closer examination of the habits and spores will help us to explain several points, and it is interesting to note the following remark anent *H. leporinus* (Fr.), a form growing on the downs :   " The spores have a *pale umber tint*,"* and Stevenson says the same.†

Again, summing up, the other genera of Agaricini number about 400 species, of which somewhat more than half are white-spored, and all typically woodland forms, except about twenty-five species of *Hygrophorus :* the other moiety have coloured spores, principally tawny, and it is not easy to generalise about them, except that most grow in the shade.

I have been able to get very little information about the colour of the spores in *Polyporei*, but in the genus *Boletus* there are about forty species, the spores of which vary in colour considerably.   Most of them are shade forms, but at least five occur in the open : their spores are ochre, yellow-brown, and green-brown.   I can only find one form with white, or rather sub-hyaline, spores—it grows in pine woods.

As regards *Polyporus*, the colour of the spores is rarely known. *P. cæsius* is said to have pale blue spores ;‡ it grows on dead firs. Several have white spores—all wood species.

Passing over a number of forms which need investigating in this connexion, we may glance at the genus *Clavaria*.   Out of more than forty species, I find fifteen with white spores, and these seem pretty

* Cooke, *loc. cit.*, vol. 1, p. 198.
† *Loc. cit.*, vol. 2, p. 78.
‡ Stevenson, *loc. cit.*, vol. 2, p. 198, quoting W. G. S. as authority.

equally divided between woods and pastures. It seems likely that the bright colours (often yellow) of the hymenophores standing upright among the blades of grass are of importance in this genus, but the matter requires looking into.

Uredineæ are all orange to orange-brown, and remarkably brilliant and well screened. Moreover, we have information as to the optical properties of their colouring matters, &c., as already stated.* It is surely a significant fact that these bright orange Fungi are especially leaf parasites, exposed to the full sunlight, direct or reflected, during their whole course of spore formation, &c.

Ustilagineæ are all dark spored; they look black in the mass, but the colour is oftener a warm brown, and seems admirably adapted to their habits. The conidial forms are developed in shade on the ground.

Ascomycetes require more examination as regards the colours of the spores, but it is well known that many asco-spores are dark-coloured or tawny, and the *Nectrias* and *Pezizas* show how thoroughly well screened the developing asci are.

The reds of the Nectrias have been examined by Bachmann and Zopf; these colours may well be correlated with the exposure of the perithecia and conidia in winter.

The dark colours of the *Sphæiaceæ* and their allies are also probably of use as screens, and the similar coatings to Sclerotia, which contain large stores of reserves, may also be mentioned.

There are certainly many more cases which seem to be explained by the hypothesis; but I will now turn to a few difficulties.

I suppose the yellow stroma of *Epichloe* sufficiently shields the asco-spores in the sunken perithecia, but it seems difficult to explain the *purple* stroma of the allied genus *Claviceps;* nor can I explain some other cases of purples and blues as yet.

Bachmann, whose work on the spectroscopic analysis of fungus pigments I have already referred to, investigated these colours from all parts of the plants concerned, and dealt with spores only, or almost only, in the case of *Uredineæ*. Consequently I cannot utilise his other results directly. At the same time it is worthy of notice, that on looking over Bachmann's forty-two absorption-spectra of colours of Fungi—including reds, violets, and orange hues—it is significant that the extreme blue-violet, from the line G onwards, is cut out entirely in all but two cases, in each of which the absorption begins on the violet side of G.

The extreme red, near the line A, seems always to be absorbed also.

Then we find differences. The red pigments of *Russula, Cladonia,*

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* Rathay thought the colours of *Uredineæ* attracted insects ('Denkschr. d. K. Akad. d. Wiss., Wien,' 1882, vol. 46).

Agaricus peziza, Gomphidius cut out most or all of the blue and green entirely, and in some cases, in fact—*e.g., Peziza sanguinea*—only allow red and a little orange to pass at all.

Now comes a curious point with regard to the *violet* pigments of *Cortinarius, Agaricas,* and *Lactarius.* According to Bachmann, they cut out all the red below the line B, and all the indigo-violet beyond the line G, together with more or less of the blue-green. Stranger still, the violet pigment of *Lactarius deliciosus* cuts out everything beyond the line D except a little blue between F and G, and transmits most of the red-orange and yellow.

Bachmann himself points out that two colouring matters, a yellow and a violet, occur side by side, and he also refers to the transient nature of some of these violet colours, *which turn yellow with age.*

Since such cases may be common, we must not accept a violet pigment without close examination, for it may be due to a mixture, or to superposition, or to other causes, and is by no means necessarily a colour screen in the sense that the orange and red pigments of others are.

The orange-yellow pigments of the spores of several Uredineæ show spectra very uniform in type. The indigo-violet from G onwards all goes out, and strong absorption bands are formed in the blue between F and G, and again at the line F. Most of the red, orange, and green are transmitted. Bachmann shows that the oily masses in these spores of Uredineæ are true fats, and the orange colouring pigments are associated with them. In connexion with the easy germination of such spores, it is interesting to note that these orange-coloured fatty drops pass into the germinal hyphæ.

As regards the colours in *Phycomycetes,* we know almost nothing of use in the present connexion. The spores and sporophores are often coloured with yellow or orange pigment, *e.g., Pilobolus, Mucor, Chytridiaceæ,* but, although Zopf says (' Die Pilze,' p. 148) these colours are also associated with fats, they still require spectroscopic investigation.

Pollen.

Another very interesting case is that of pollen, the microspores of the flowering plants. It is obvious that the pollen grains are often exposed for many hours to intense insolation in the concave cup of the corollas of many flowers and on the bodies of bees and other sun-loving insects. Now a pollen grain is just such a storehouse of fatty oils as, according to my view, ought to be protected from the destructive rays, and in the vast majority of cases the deep orange colours of these spores accord well with the purpose suspected.

Even in the very few references to the colours of pollen that I can find, however, there are cases which at first sight seemed to present great difficulties.

Fischer ('Beitr. zur Vergl. Morphol. d. Pollenkörner,' Breslau, 1890) says that most colours *can* be found in pollen, and that the pigment usually, but not always, occurs in the *exine, i.e.*, the outer coat of the spore, and not in the contents, a fact very well in accordance with the view that the colour acts as a screen, though Fischer regards the coloured oily body as a sticky medium to facilitate the adherence of the body to the insect.

Among the comparatively rare pollen which is not yellow or orange the following may be mentioned :—

Pollen is *white* in *Actœa, Richardia*, and other Aroideæ, and some species of *Iris*, as Professor M. Foster informs me. In the last cases, however, Professor Foster adds that the white is a dead one. *Actœa* s a shade plant, and in Aroideæ the rule is for the pollen to be formed and remain in the tube of the spathe, whence these cases may not really be exceptions.

The *red* pollens of *Lilium chalcedonium* and some other Lilies, *Verbascum, Clerodendron cermum*, and *C. Thomsoni* (Fischer), and some Geraniums, and the *red-browns* of other Lilies and of *Æsculus*, passing into deeper *browns* in some Poppies, are not inconsistent with their being screens against blue light.

The cases which promise serious trouble, however, are the *blue* pollens of certain *Irises, Epilobiums*, some *Polemoniacœa, Scilla sibirica;* some Poppies (*e.g., Papaver orientale* and *P. bracteatum*) are almost pure blue, according to Fischer. '

Certain species of *Linum, Anemone, Althœa, Agapanthus, Gilia, Echium*, and a few *Caryophyllaceœ* are also said to be of various shades of blue or violet, and they appear as witnesses against the above generalisation. At the same time it must be borne in mind that blue pollen is rare, that we know little or nothing of the spectroscopic and other optical properties of these pigments, and that it may turn out that special peculiarities in the biology of the plant are correlated with this colour.

Fischer mentions (*loc. cit.*, p. 10) some species of *Geranium*, the pollen of which appears steel-blue ; this colour is due to the presence of a very volatile blue oil, which rapidly disappears as the pollen is exposed, and leaves the latter *dark yellow* in colour. If many such cases occur, blue pollen may turn out to be easily explained.

Other Instances.

It is obvious, however, that if the foregoing explanation of colour screens is true, it will have a much wider application than I have as yet given it. The following are probably examples in point :—

Sporangia and spores of all kinds among the vascular Cryptogams are usually some tint of orange or sienna—*e.g.*, Ferns, *Lycopodium*,

Selaginella, &c. The tufts of archegonia and antheridia in Mosses[*] are often orange to red.

Nor are Algæ wanting in good examples. I take it the oögonia and antheridia of the Characeæ are cases in point; and the yellow pigment accompanying the antherozooids and oöspheres of *Fucus* as they ooze from the conceptacles at low tide probably serves as a colour screen.

The red stigmas of *Corylus* would also seem to be one of many similar cases in point; but, unless the Gramineæ have their plumose stigmas rapidly withdrawn into the shadow of the paleæ, they would seem to be witnesses against me.

The matter will probably not end with such cases as I have cited, and I am strongly inclined to regard chlorophyll as serving the part of a colour screen in the sense indicated, as well as that of an instrument[†] of assimilation. This is by no means the same as Pringsheims's screen theory of chlorophyll, but it may well be that the great absorption in the blue and violet has a screen effect.

Some Practical Bearings of the Results.

The establishment of the *fact* of the bactericidal and fungicidal action of light, dating from Downes and Blunt to now, enables us to see much more clearly into the causes of several phenomena known to practical agriculturists, foresters, hygienists, &c.

It helps to explain, for example, why the soil of a forest should not be exposed to the sun, a dogma long taught in schools; it will also affect our way of regarding bare fallows. It has already been shown how important is its bearing on the purification of rivers, and the reasoning obviously applies to dwellings, towns, &c. I regard it as probably explaining many discrepancies in the cultures of Schizomycetes and Fungi in our laboratories, and as having a very important bearing indeed on the spreading of plant epidemics in dull weather in the summer, and no doubt this applies to other cases on which I can speak with no authority.

That sunshine has something to do with the rarity of bacterial diseases in plants now seems quite as probable as the currently accepted view that the acid nature of the latter accounts for the fact.

If that part of the chlorophyll which absorbs the blue-violet is a screen to prevent the destruction of easily oxidisable bodies, as they are formed in the chloroplasts, we may reconcile several old experimental discrepancies—*e.g.*, the behaviour of plants under bichromate and cupric oxide screens.

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[*] The calyptra may be of service as a screen in another way.

[†] Elfving also (*loc. cit.*, p. 54) hints that the chlorophyll may serve as a screen in so far as to prevent certain destructive metabolic actions in synthesis.

Presents, February 16, 1893.

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February 23, 1893.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and
Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks
ordered for them.

The following Papers were read :—

I. "On the Mimetic Forms of certain Butterflies of the Genus
   Hypolimnas." By Colonel C. SWINHOE, M.A. Communi-
   cated by Professor E. RAY LANKESTER, F.R.S. Received
   January 28, 1893.

(Abstract.)

The object of this investigation is to study the changes undergone
by the species of a small group of Butterflies as they are traced from
one locality to another, and to ascertain the bearing of these facts
upon the theory of mimicry.

We find the representatives of the Indian *Hypolimnas bolina*
in a long list of localities in Malaya, Polynesia, and Africa : the
local representatives differ from each other and from the Indian form,
but they agree in possessing in one or both sexes a more or less
superficial resemblance to some conspicuous species belonging to a
specially defended group and inhabiting the same locality; the same
is true of the three forms of the female of *Hypolimnas misippus.*

The facts afford the most convincing evidence of the truth of the
theory of mimicry enunciated by H. W. Bates.

The study of these numerous but closely-related forms belonging
to the genus *Hypolimnas* also throws light upon such interesting
questions as :—

(1.) The special liability of the female to become mimetic.

(2.) The ancestral form from which the various mimetic varieties
have been derived.

(3.) The mimetic resemblance to different species in the same
locality.

(4.) The divergent conditions under which mimicry appears in
closely-related species.

(5.) The relation between selection and variation in the production
of mimetic resemblance.

II. "Upon the Action of Gravity on *Bacterium Zopfii.*" By
Rubert Boyce, M.B., Assistant Professor of Pathology,
University College, London, and A. Ernest Evans, M.B.,
C.M., Glasgow.   Communicated by Professor Horsley,
F.R.S. Received February 7, 1893.

(From the Pathological Laboratory of University College, London.)

(Abstract.)

Bacterium Zopfii was accidentally discovered by Kurth, in Zopf's
laboratory, in the alimentary canal of the fowl. He showed it to be
pleomorphic, and observed its radiate mode of growth, and, in addi-
tion, its great tendency to form spirals. It was rediscovered by
Crookshank in Johne's laboratory in Dresden; this observer obtained
it from the air, and likewise noted the characteristic pinnate mode of
growth. He named it *Bacterium figurans*, on account of its figured
appearance upon the solid nutrient media. We obtained it from a
case of otitis media in the cat, in London. These observations show
that the organism is probably widespread.

Mode of Growth of the Bacterium.—On gelatine it forms, under
certain circumstances, a feather-like or pinnate growth upon the
surface and in the gelatine, the branches of which are directed
upwards at about an angle of 45°. Under no circumstances (accord-
ing to our observations) does it form a pinnate growth upon agar or
potato.

Action of Gravity.—The symmetrical pinnate growth could not be
obtained in gelatine tubes which were kept horizontal or nearly so,
whilst similar tubes kept vertical or nearly so exhibited the character-
istic growth. A certain scale of symmetry could be obtained by
placing the tubes at various angles between the vertical and hori-
zontal. If a tube in which a pinnate growth had been obtained was
completely reversed, a new pinnate growth might be seen to cross
the first, but it was not so marked as the latter. These observations
showed that *Bacterium Zopfii* was markedly susceptible to position,
and that the pinnate growth could only be obtained in or about the
vertical. They pointed strongly to the influence of the action of
gravity.

Behaviour on the Clinostat.—If the force of gravity acting upon the
culture tube in the one position were prevented by slowly revolving
the growth in the vertical at rates from one revolution in one minute
to one in one hour, then the pinnate growth could not be obtained.
We assumed, therefore, that *Bacterium Zopfii* was subject to the
action of gravity, and that it was negatively geotropic. But if it were

negatively geotropic, then it ought also to assume a *centripetal* direction if rapidly revolved.

Action of Centrifugal Force.—We repeated Knight's experiment, communicated to the Royal Society in 1806, and revolved the culture tubes in the horizontal at the rate of three to five revolutions per second, and we obtained most perfect pinnate growths, similar to those obtained by growing in the vertical.

The Cause of the Pinnate Growth.—The growth is very rarely ortho-geotropic; there is a force hindering this, which we believe to be the *resistance of the gelatine.* Although the pinnate growth appears superficial, this is not so; the filaments tend to dip slightly or deeply into the substance of the gelatine. It is observed that where they penetrate deeply into the gelatine they are almost horizontal at the bottom of the gelatine, whilst nearly vertical at the top of the gelatine column. These differences are still more accentuated when the tubes are centrifugalised.

Circumstances which Favour or Retard the Symmetrical Growth.—It is essential that the gelatine be not too stiff; if it is hard, an irregular superficial or an imperfect pinnate growth obtains. We employ a freshly prepared 8 to 10 per cent. nutrient gelatine. A slight variation in the *reaction* on the acid or alkaline side of neutrality makes no appreciable difference. *Temperature* has a very important effect; symmetrical growths are with difficulty obtained at low temperatures; the most suitable is that between 20° C. and 21° C. *Carbonic acid* hinders the symmetrical growth; oxygen favours it. Thus, if a streak culture on gelatine is placed in an atmosphere of CO_2, at the end of three days there is only a thick streak along the line of inoculation; if the tube is then transferred to oxygen, a symmetrical growth develops in the course of twelve to twenty-four hours. We have not yet observed any difference in the growth in the colours of the *spectrum*.

Not only has the hardness of the gelatine a marked effect upon the pinnate growth, but in addition the *thickness* of the gelatine substratum is important. Thus, we never obtained pinnate growths in our method of plate cultures on microscope slides. Even in Petri boxes, with a thickness of gelatine varying from $\frac{1}{4}$ inch to 1 inch, the *growth upon the surface* often tended to be irregular, but in these cases the filaments which grew deeply into the gelatine assumed the pinnate form, with the exception, as before mentioned, that the lowermost branches were more horizontal than those at the top. Thus, to obtain a pinnate growth upon the surface, the test-tube is the best culture glass.

Microscopic Characters of the Growth.—The organism is pleomorphic; filamentous, bacillary, bacteroid, spiral, and coccal form phases being met with. There is also a motile stage, in which bacillary, bacteroid,

and coccal forms predominate; there appears to be an absence of the motile spiral form. In the motile phase there is a very slight lique-faction of the gelatine, but for the rest there is no liquefaction of the gelatine. In addition to the differences of form of the separate elements, there appear certain phases in the grouping of the bacteria. Thus, following Billet, we divide our growths into *filamentous, disassociated, skein,* and *zoögleaform phases.* The *filamentous form* is especially met with in gelatine, and in the pinnate growths the filaments are at first very long and hyphal-like, and are rapidly growing, but segmentation follows early. If branching occurs it appears to take place as in *Cladothrix,* by displacement of segments. In the *disassociated phase* the segments break away from one another, and may become motile. The *skein phase* is met with on hard gelatine and on agar; it is a very superficial growth, and the filaments form dense plaits; it is not met with in geotropic growths. The *zoögleaform phase* is very characteristic, and usually rapidly follows upon the filamentous. In the filaments and segments multiplication occurs either at scattered points in the chain or throughout its length, and the segments remain united by a common cementing substance; the forms assumed by these aggregates are very beautiful, and are to be met with in the geotropic growths.

The Spirillation.—The undivided or segmented filaments and the zoögleaform masses have a marked tendency to twist, especially in the pinnate gelatine cultures. The majority of the twists are in the opposite direction of the hands of the clock. Kurth noted the spirillation, and attributed it to the resistance of the gelatine; but as in the higher plants, perhaps here, the action of gravity is also a factor. This consideration also opens up the meaning of the spiral form in the bacteria.

Methods of Fixing and Staining employed.—Our preparations, from which the micro-photos were taken were obtained from plate cultures made in the following manner:—A micro-slide in a large sterilised test tube is coated with a thin surface of sterilised gelatine by means of a balloon pipette. After solidification the surface is inoculated with a streak. The growth may be examined either unstained or after fixing and staining. To fix, the slide is withdrawn from the test-tube and placed in dilute alcohol, and then dried; any stain can then be employed, and the preparation mounted in Canada balsam.

III. "On *Dischidia Rafflesiana.*"   By Percy Groom, M.A. Communicated by S. H. Vines, F.R.S.   Received February 1, 1893.

The Function of the Pitchers.—The pitchers contain living ants and acari, small quantities of insect remains, considerable amounts of earth, humus, and water; but all these bodies and substances are not found in each pitcher. The earth and humus, though partially brought to the pitchers by the agency of rain-water, are mainly conveyed thither by ants, which nest within the pitchers. That the roots within the pitchers utilise these solid matters is suggested by the following facts, observed on living plants in the Botanic Garden, Singapore :—1. The roots are well developed in pitchers containing a rich store of earth and humus. 2. Sometimes these solid substances are arranged up the whole of one side of the cavity of the pitcher, in which case the roots are more strongly developed on the same side; these phenomena, in some cases at any rate, could not have been occasioned by water having previously been distributed in the pitcher in the same manner. 3. Particles of earth are found clinging closely to the root-hairs, which are always well developed in the presence of solid substances. Actual experiments on living plants prove that the pitcher-roots can absorb liquids. The pitchers are not mere water-reservoirs; they are depositories for solids from which, by means of the roots within the pitchers, the plant derives part of its nutriment. Probably the evolution of the pitchers has been, to a great extent, determined by the myrmecophilous habits of the plant.

Structure of the Roots.—In the young state there is an epidermis, many of the cells of which grow out to form root-hairs. The hairs of the climbing roots are more numerous on the side towards the supporting stem or branch of the host-plant ("ventral side"), where they form a mycelium-like weft. The hairs of the roots within the pitchers are more uniformly distributed, but are numerous and short at regions where the root is in contact with the pitcher-wall or with another root. The hairs of the pitcher-roots, and those on the ventral side of the climbing roots, persist for a long time, and are cuticularised: elsewhere the epidermis disintegrates. The epidermoidal layer or exodermis is made up of cells, the walls of which are cuticularised and lignified. The outer walls of the passage-cells of this layer are thick, often possess pits, and are traversed by what may be either radial canaliculi or radial rods of a substance differing from the rest of the wall. Close beneath the exodermis of the pitcher-root, usually separated from it by one layer of cells, are about six bands of sclerenchyma; these bands are absent from the climbing

roots, or only feebly developed on the dorsal side. The cortical layer next to the exodermis constitutes itself into a cork-cambium; and in the pitcher-roots a secondary cork-cambium arises within the bands of sclerenchyma. Cork is not formed on the distal parts of the pitcher-roots, nor within the most ventral portion of the climbing roots. Sometimes there is a group of large wood-vessels in the ventral portion of the vascular cylinder.

The points especially worthy of note in the roots of *Dischidia Rafflesiana* are—

1. The early cuticularisation of the root-hairs, and the long persistence of these structures, which, in climbing roots, remain to function as anchoring threads.
2. The curious passage-cells of the exodermis, which do not possess thin cellulose-walls.
3. The precocious development of cork to prevent excessive loss of water.
4. The radial structure and large sclerenchyma-bands of the pitcher-roots.
5. The dorsi-ventral structure of the climbing roots, as revealed in the formation of root-hairs and cork; also as seen in the structure of the cortex and even of the vascular cylinder.

IV. "The Har Dalam Cavern, Malta, and its Fossiliferous Contents." By JOHN H. COOKE, F.G.S. With a Report on the Organic Remains, by ARTHUR SMITH WOODWARD, F.L.S., F.G.S., F.Z.S. Communicated by HENRY WOODWARD, LL.D., F.R.S., V.P.G.S. Received February 2, 1893.

[Publication deferred.]

Presents, February 23, 1893.

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"On a Meteoric Stone found at Makariwa, near Invercargill,
New Zealand." By Professor G. H. F. ULRICH, F.G.S., of
the University, Dunedin, New Zealand. Communicated
by Professor J. W. JUDD, F.R.S. Received December 14,
1892,—Read February 2, 1893.

Introductory.

The stone under notice was not seen to fall, but the following description regarding the site of its discovery, its mineral character, and structure can leave no doubt of its being of meteoric origin.

Towards the end of the year 1886, when a large party of mining prospectors were preparing, with Government aid, for departure to the Big Bay district, west coast of Middle Island, Mr. Th. Fenton, a student of the Dunedin University School of Mines, was sent to Invercargill, where the party assembled, to instruct those of the men who desired it in rough assaying for gold and the use of the blowpipe. On the occasion of one of his lectures, he received from a Mr. Arch. Marshall, for examination, a piece of stone which, from its weight and appearance, was supposed to be something out of the common. Mr. Fenton made a rough qualitative analysis of a sample of the stone, and on finding strong reactions for nickel, thought it of sufficient interest to preserve the several small fragments remaining of the piece received from Marshall and to bring them with him to Dunedin, where he placed them at my free disposal. One of these fragments I devoted to the preparation of a number of thin sections

sliced in different ways, and the microscopic examination of these convinced me at once of the meteoric character of the stone. After this, I made every endeavour, by correspondence and ultimately travelling to Invercargill, to ascertain the exact locality where and under what circumstances the stone was found, and to obtain more of it if possible; for the surface outlines of the remaining fragments clearly indicated that it must originally have been of considerable size. The results of my investigations in these directions are the following :—In the year 1879, at the completion of the connexion of the railway line Invercargill—Winton and the branch line Makariwa —Riverton, two workmen, the brothers Arch. and I. Marshall, while engaged in removing a clay bank at Makariwa Junction, found in the clay, about 2½ feet from the surface, a roundish stone, which at once attracted their attention, on account of its weight and because of the fact that in the clay-covered plain surrounding Makariwa Junction stones of any kind are a great rarity. They broke the stone with the pick and, finding the inside of different aspect from the outside, took the fragments home, and, experimenting with them, discovered that they affected the magnetic needle. With the intention of having the stone some day further examined, the pieces were kept as curiosities; but, being unsightly, they were kicked from one corner of the room into the other, and specimens were also occasionally knocked off for friends interested in the find. Mr. Arch. Marshall, who gave me these particulars, told me, on further inquiry, that the stone, when originally found, had a knobby, roundish shape, was of the size of a large man's fist, or perhaps a little larger, and might have weighed between 4 and 5 lbs. The exact place of the find was about half way between the railway station on the Winton line and the station master's house, some 20 feet from the line of rails. A search by Mr. Marshall for another piece of the stone which he thought was still somewhere about the premises at the time he gave the one to Mr. Fenton proved, unfortunately, unsuccessful, and the only secured remnants of this meteorite are the two pieces sent with this paper and another small piece divided between the Dunedin and Wellington Museums.

Macroscopic Character of the Stone.

All the fragments show portions of a yellowish-brown, rather soft, and earthy decomposition-crust, from 2 to 3 mm. in thickness, which evidently enveloped the entire stone when found. The undecomposed rock beneath this crust is very finely granular, rather hard and firm, and in fresh fracture dark greenish-grey, finely-mottled, light brown, and most nearly resembles fine-grained picrite in appearance; it slightly darkens, however, on exposure to the air, and shows on old fracture planes a dark brown, ferruginous glaze. Even under a strong

magnifying glass the nature of the minerals composing the rocky part of the stone cannot be determined, but tin-white metallic particles, abundantly present, can easily be distinguished; one also sees small, irregularly shaped pores or cavities, some partially filled with a brown ferruginous substance.   After the fragments had lain untouched for some time in a closed box, there were also observed under the magnifying glass, on parts of the fresher-looking surfaces, very small drops of a dark brown fluid, which, however, spread and dried up before a micro-chemical test could be made.   This fluid seems to originate the ferruginous glaze before mentioned, and may possibly consist of chloride of iron.

Microscopic Character of the Stone.

For the purpose of microscopic examination, seven thin sections were available, which proved the stone to belong to the " chondrites," or, according to Wadsworth, to the variety " saxonite " of the peridotites, and to be composed of the following minerals:—Olivine, enstatite, glass, a substance resembling glass, nickel-iron, troilite, and black opaque grains, probably magnetite.   All the sections are, besides, more or less strongly flecked and stained with brown hydrous ferric oxide, originated, no doubt, through oxidation of the iron minerals.

Olivine.—This is the predominant mineral, and has a faint greenish-yellow colour in common transmitted light, but shows brilliant polarisation-colours between crossed nicols.   It appears partly in well-defined chondrules, partly in larger and smaller grains of crystal-like or irregular outlines, closely packed together—the larger ones in some cases much, in others little, fractured; also in columnar bodies, some short and stout, others long and narrow, and sometimes curved, and all generally much fractured transversely—most fractures running at right angles to the length of the crystals, others obliquely, and from both sides.   The extinction between crossed nicols of these columnar bodies is mostly parallel to their longitudinal extent, in cases more or less oblique, and it is rarely quite complete throughout, as small brightly coloured specks are generally observable here and there within the darkened body.   The chondrules show various modes of internal structure.   One kind is very fine granular, of dull-greyish or dusty aspect, and extinguishes pretty uniformly throughout, only some promiscuously distributed bright grains shining out.   Around these chondrules large clear olivine grains, sometimes all of uniform, sometimes each of independent, optical orientation, are frequently arranged ring-like, and so that the divisional joints stand rudely radial (see figs. 3 and 6).   This radial arrangement of the joints respecting grains is also observable, however, around groups of *several larger grains* as centre.   A rare and pretty variety of the

kind of chondrule under notice is shown in fig. 1 (only one example was found in the several sections). In this the mass of the chondrule is constituted partly of fine granular dusky olivine, partly of an assemblage of large clear grains of this mineral, and has a ring of small clear grains of the same surrounding the fine granular part, whilst there is a border of dark grains of iron minerals around the whole. In another kind of chondrule the mass is divided or fractured by strong dark cracks into rudely parallel, columnar, and much cross-fractured portions, which generally extinguish between crossed nicols parallel to their length. The divisional cracks are sometimes so close together as to impart to the whole a coarsely fibrous structure. Some large chondrules occur also, exhibiting several systems of parallel cracks which meet at various, generally obtuse, angles, and in others, again, the cracks are eccentrically radiating. All the sections show patches of roundish or quite irregular outlines, which have a striking porphyritic structure, arising from the prevalence of an extremely fine granular dusty base, through which are distributed comparatively large clear crystals and irregular grains of olivine, and also occasionally enstatite; the base is probably also composed of a mixture of granules of these two minerals. Serpentinisation of the olivine was not observable in any of the sections.

Enstatite.—Besides appearing in the porphyritic mode just mentioned, this mineral occurs also sparingly distributed amongst the larger olivine grains and more abundantly in well defined chondrules. In the first case, it forms either columnar bodies, sometimes broad, but mostly narrow, or irregularly outlined grains—in fact, its forms are very similar to those of the olivine; and, as its colour is also next to identical with that of the latter—if anything, that of enstatite being fainter—the two minerals can hardly be distinguished in common transmitted light. In polarised light, between crossed nicols and on rotation of the stage, the difference is, however, very marked in that the colours of the enstatite are of a low order—only various shades of yellow and grey—contrasting strongly with the brilliant red, blue, and green colours of the surrounding olivine. One has to guard, however, against mistakes in this respect, as olivine grains cut at, or nearly at, right angles to one of the optic axes—of which there are examples in every section—show similar colours on rotation of the stage. The employment of convergent polarised light solves this question at once, and, in most cases, strongly marked cleavage-cracks are seen in enstatite, parallel to which extinction takes place, whilst generally, also, a finely fibrous structure becomes apparent on slightly lowering the polariser with its top lens. The chondrules are of rather dusty aspect throughout, or in cases only around the circumference, and have mostly an eccentric *or fan-like fibrous structure, as shown respectively in fig. 2, lower margin, and fig. 5, upper margin. On*

rotation of the stage, the extinction is undulating, approximately parallel to the fibre-lines, but a multitude of minute granules and lath-shaped bodies always shine out brightly along the darkened fibres. As in the case of the olivine, none of the sections show alteration of the mineral to serpentine.

Glass and a substance resembling Glass.—These require to be described together, because of their mode of occurrence and their likeness in common transmitted light. They are both perfectly colourless, transparent, and occur pretty abundantly, though mostly in small particles, filling generally larger and smaller crevices between the grains of olivine or of olivine and enstatite; their limpidness, combined with absence of colour, rendering them easily distinguishable from these minerals. The difference between the two substances consists in the one—the glass—being perfectly isotropic, the other anisotropic. The large finely-dotted patch near the centre of fig. 5, and the similarly dotted belt, enclosing several clear spaces, which extends in fig. 6 from the lower edge through the centre towards the upper edge, consist largely of the real glass. They show between crossed nicols a dark background, as it were, speckled with innumerable bright granules, polarising in fine colours, therefore, no doubt, olivine ; and though on rotation of the stage the dark ground brightens up granular-like in many places, still there are numerous spots which remain perfectly dark throughout a complete rotation. In fig. 4 a large rather ill-defined olivine chondrule is delineated, in which the clear (undotted) parts on the right and left of, and also within the main part of the olivine near the upper edge, consist all of the anisotropic glass-like substance. For whilst between crossed nicols each separate particle shows dark and light spots—the light ones extinguish and the dark ones brighten up on rotating the stage—the extinctions are not, however, sharply defined, as in the former case, but cloudy or undulating, and there is also no chromatic polarisation. All things considered, it is very probable that the anisotropic substance simply represents glass in various stages of devitrification.

Nickel-Iron.—This is indicated in the figures by the dotted and cross-shaded areas, and is easily recognisable in the sections in reflected light by its metallic silvery-white lustre. As seen by comparison of the figures, it occurs in relatively large proportion, but is rather unequally distributed through the stone. The particles are highly magnetic and vary from very minute specks to such as approach a grain in weight. One such, for instance, was found on grinding a sample of the stone for chemical analysis. Larger particles sometimes enclose small grains of olivine, as shown in fig. 2 near the left-hand edge, and there occur also portions in some of the sections in which the iron forms a kind of rude network the meshes of which are occupied by olivine.

Troilite.—Rather small, yellow, and bronze-coloured metallic specks observable in the sections in reflected light belong, no doubt, to this mineral, but they are very scarce, and the aggregate percentage indicated by them, as present in the stone, would be very small indeed. According to the quantity of sulphur obtained in a preliminary chemical examination by Mr. James Allen, a former student of the Royal College of Science, London, the stone should, however, contain an appreciable percentage of troilite; and it is, therefore, very probable that some of the black grains, without metallic lustre, observable in the sections, belong to it, being partly oxidised and coated over by hydrous ferric oxide. Dark dull spots on the surfaces of many particles of the nickel-iron, or closely joined therewith, are very likely also due to this process.

Magnetite.—The majority of the rather abundant dark grains, some of which show square, others hexagonal, outlines, belong doubtless to this mineral. The distribution of the grains is very irregular; in some parts they are abundant, in others rather scarce; their closest aggregation is generally in the borders they form around chondri of olivine. Chromite may also be present, but only in very small quantity, as the chemical examination of the stone afforded only a trace of chromium.

Specific Gravity.

The specific gravity of the stone, determined from several small fragments, varied between 3·31 and 3·54: the variation is, no doubt, due to the unequal distribution of the metallic particles.

Results of the Chemical Examination of the Stone.

A chemical examination has since been undertaken by Mr. L. Fletcher, of the Mineral Department, British Museum. The examination is not yet finished, but the results already obtained by him indicate that the percentage mineral composition is approximately expressed by the following numbers:—nickel-iron, 1; oxides of nickel and iron, 10; troilite, 6; enstatite, 39; olivine, 44. The details of the examination will be communicated later.

Description of the Figures.

All the figures are drawn in ordinary light from thin slides cut in different directions from one of the fragments of the stone.* The nickel-iron particles, as seen in reflected light, are indicated by

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* The small pieces remaining after the slicing were used for my preliminary chemical examination and the determination of the specific gravity.

dotting and cross-shading. Figs. 1—5 are magnified 25 diameters, fig. 6 is magnified 50 diameters.

Fig. 1.—This shows a nearly circular chondrule—the finest seen in any of the sections. It consists throughout of olivine : the upper very fine-granular part is somewhat dull or dusty, the lower coarse-granular part is clear, but some of the cracks are filled with dull-brown ferruginous matter. Around the circumference of the fine-granular part there is a rim of small clear grains of olivine, the divisional joints of which stand mostly radial; and around this rim again, also surrounding the coarse-granular part, there is a border of small, black grains of magnetite. The fine-granular part and its encircling rim of radial grains, as well as most of the grains of the coarse-granular part, have the same optical orientation,

FIG. 1.

as they show pretty uniform simultaneous extinction between crossed nicols.   In one of the grains of the coarse-granular part at the point indicated by a dotted line and the letters (*oa*) an optic axis is visible in convergent polarised light. What is seen outside the chondrule consists of an aggregate of larger and smaller grains, and several very fine granular patches of olivine showing aggregate polarisation and enclosing a number of particles of nickel-iron and dark grains, some of which, of dull-bronzy lustre, may be troilite.   The two large particles of nickel-iron near the upper edge of the figure are so connected by and with a dull dark substance as to render it very probable that this substance is also nickel-iron, which, through oxidation, has lost its metallic lustre.

Fig. 2.—This shows on the lower margin, a little to the right, part of a large chondrule of enstatite rendered fibrous by fine eccentric *striæ, and rather* densely filled with fine dark dust.   Between crossed *nicols on rotation* of the stage its polarisation colours are of a low

order, varying between light yellow and bluish-grey, and extinction takes place in an undulating manner, mostly parallel to the fibration. In reflected light it looks yellowish and opaque. The remainder of the section consists of olivine, in part finely granulated, in part in larger and smaller grains, sprinkled with particles of nickel-iron and dark grains of iron ore. At the left-hand margin, nearly enclosing two particles of nickel-iron, is a large grain of olivine which is divided

Fig. 2.
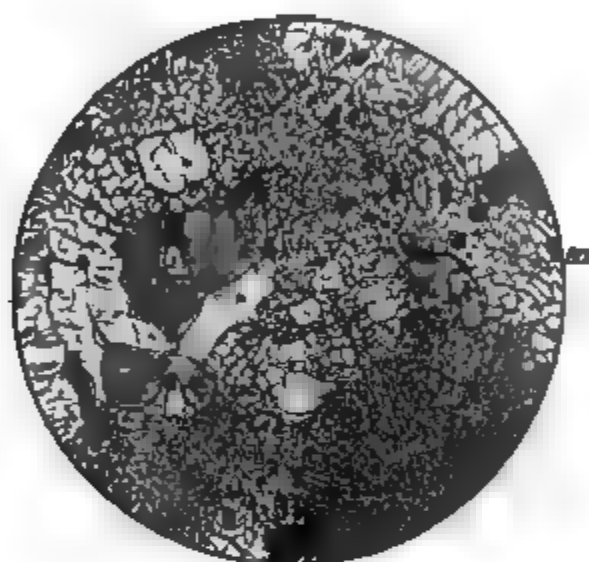
into columnar portions by rudely parallel fractures in the line of which extinction takes place between crossed nicols. In the finely granulated centre of the figure, scattered larger grains of olivine produce quite a porphyritic appearance which becomes more pronounced through the different optical orientation and the fine polarisation-colours of these larger grains. The largest particle of nickel-iron encloses a grain of olivine in the centre; and a deep indentation of another particle of the metal, seen close above the enstatite chondrule, is occupied by an olivine grain showing an optic axis (oa) in convergent polarised light. Some particles of the nickel-iron have narrow, dark margins, produced, no doubt, by oxidation.

Fig. 3.—Near the centre of the figure is represented a fine granular chondrule of olivine surrounded by a rim of clear grains of this mineral, the divisional joints of which stand generally radial. The optical orientation of this rim and that of the centre part are different, the latter uniformly extinguishing between crossed nicols when the rims of the grains are bright, while all the grains become dark simultaneously when the centre part is bright. Below this chondrule, near the lower right-hand margin, is a rather rudely-radial arrangement of olivine grains around a centre part consisting of four grains of this mineral, but in this case the centre grains extinguish simultaneously *with the surrounding ones,* thus proving the whole to be a large

FIG. 3.

grain cracked in the manner shown.   The oblong grain near the lower margin indicated by a dotted line and the letter *e* is enstatite.   It shows strongly marked cleavage-cracks oblique to its outlines, polarises in low colours, ranging between light brown and grey, and extinguishes parallel to the cleavage-cracks.   The remainder of the section is constituted of finely and coarsely granulated olivine, polarising aggregately, and enclosing a number of particles of nickel-iron and dark grains, the largest one of which, near the centre of the section, is probably oxidised nickel-iron, as it shows minute metallic spots in reflected light.   A porphyritic structure, similar to that noticed in the previous section, is seen in the finely granulated olivine portion along the left-hand margin, the larger olivine grains, each of independent optical orientation, shining out brilliantly in polarised light from the duller fine-granular part.   In a grain, indicated by (*oa*), near the upper margin, a fine optic axis is disclosed in convergent polarised light.

Fig. 4.—This shows a large chondrule, mainly composed of olivine, rather rudely defined by a border of particles of nickel-iron and dark grains, most of the former occurring around the left-hand margin. The portions of this chondrule left clear, *i.e.*, unshaded, consist of the water-clear, doubly refracting substance, resembling glass.   A large amount of this occurs along the right-hand margin, smaller portions around the left-hand margin, and within the mass of the olivine, and it also fills the large rudely-parallel longitudinal fissures, and some large irregular transverse cracks by which the latter is divided. Whilst portions of it are perfectly clear between crossed nicols, others are dark; but on rotation of the stage the extinction travels in an undulating or cloudy manner over the light portions, and those dark before become light.   The contrast between it and the olivine is especially well marked during the rotation, as the latter mineral, *though much stained* with hydrous ferric oxide, polarises in splendid

Fig. 4.
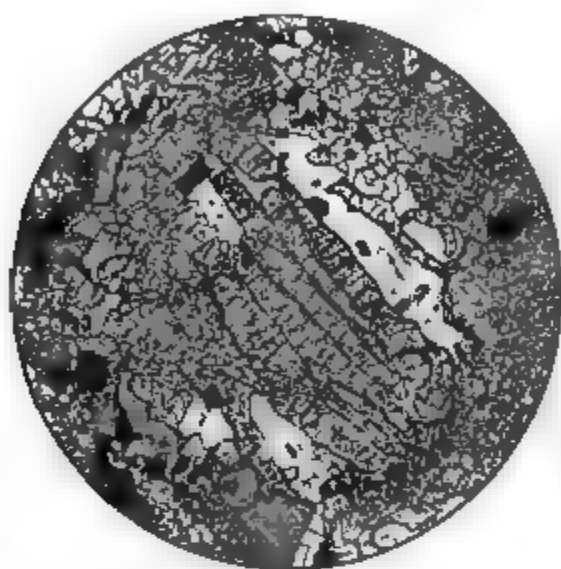
colours, and when its extinction takes place—which is in the line of the fissures dividing it into columnar bodies—the glassy substance in most of the fissures and cracks shows out perfectly clear and bright. The very finely granular part which bounds the chondrule on the right-hand margin and contains a number of larger grains, is also olivine, judging from the polarisation-colours. Some of the nickel-iron particles show dark spots and narrow dark rims, due to oxidation of the metal.

Fig. 5.—On the upper margin of this is shown part of a large chondrule of enstatite, which is finely fibrous, with the fibre-lines disposed in a fan-like manner. Around the margin it looks minutely granular and somewhat dusty; in the centre it is clear. Between crossed nicols it polarises in shades of yellow and grey, and extinguishes in the clear centre part parallel to the fibre-lines, whilst in the marginal dim part the extinction is cloudy. The large, finely-dotted patch near the centre of the section consists of a mixture of

Fig. 5.
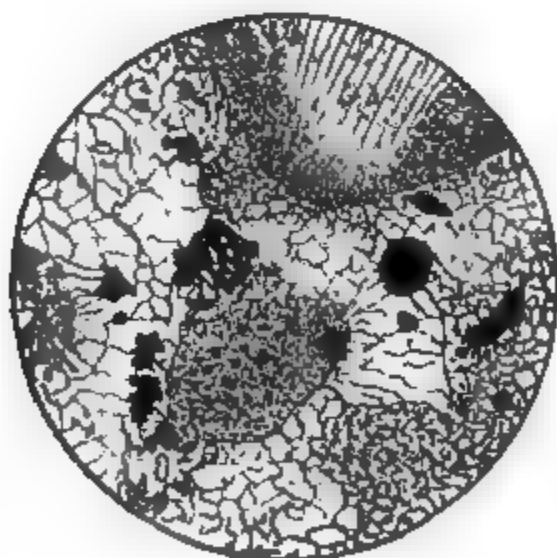
clear, isotropic glass and granularly devitrified anisotropic glass, and is full of minute, dark grains, probably of iron ore. The portion below the dotted part, near the edge of the figure, is mostly true glass and free from oxide of iron, but encloses a few small grains of olivine. The remainder of the section is occupied by finely and coarsely granulated olivine, strongly stained in places by brown hydrous ferric oxide, and there are also scattered dark grains—some rather large—in one of which, on the left of the centre of the figure, reflected light reveals numerous fine silvery-white metallic specks of nickel-iron. This is the only direct evidence of the presence of this metal in the section, but it is very probable that amongst the other large dark grains one or more consist of it to some extent, being rendered dull non-metallic-looking by oxidation.

Fig. 6.—This is drawn under twice the magnifying power used for the other figures, in order to show more clearly an occurrence of intermingled isotropic and devitrified anisotropic glass. The mass extends from the lower margin of the figure between a particle of nickel-iron on the right and an olivine chondrule on the left, upwards through the centre to the large particle of nickel-iron at the upper margin, being wider both at the top and bottom than in the centre. It is crowded with small, yet well-defined, grains of olivine, but

FIG. 6.

adjoining the lower particle of nickel-iron, and near the upper one, there are irregularly outlined portions quite free, or nearly free, from these grains, in which the true glass can plainly be distinguished from the granularly devitrified material between crossed nicols, on rotation of the stage of the microscope. Besides this mass of glassy substance, the particles of nickel-iron, and a few black grains, only olivine is represented in the section ; the fine granular chondrule on the left having for the greater part a rim of larger grains, the *divisional joints of which* stand closely radial.

March 2, 1893.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates for election into the Society were announced, as follows:—

Bateman, Sir Frederic, M.D.
Bateson, William, M.A.
Beevor, Charles Edward, M.D.
Boulenger, George Albert.
Bourne, Professor Alfred Gibbs, D.Sc.
Bradford, John Rose, M.D.
Brennand, William.
Burnside, Professor William, M.A.
Buzzard, Thomas, M.D.
Callaway, Charles, D.Sc.
Callendar, Hugh Longbourne.
Carter, Robert Brudenell, F.R.C.S.
Cheyne, William Watson, F.R.C.S.
Clarke, Sir George Sydenham, Major R.E.
Clowes, Professor Frank, D.Sc.
Darwin, Leonard, Major R.E.
Davis, James William, F.G.S.
Dreschfeld, Professor Julius, M.D.
Dunstan, Professor Wyndham R., M.A.
Edgeworth, Professor Francis Ysidro, M.A.
Elgar, Francis, LL.D.
Eliot, John, M.A.
Ellis, William, F.R.A.S.
Etheridge, Robert, F.G.S.
Ewart, Professor J. Cossar, M.D.
Gairdner, Professor William Tennant, M.D.

Harcourt, Professor Leveson Francis Vernon, M.Inst.C.E.
Harker, Alfred, M.A.
Hendley, Thomas Holbein, Surgeon-Major.
Hickson, Sydney John, M.A.
Hill, Professor M. J. M., M.A.
Hinde, George Jennings, Ph.D.
Hobson, Ernest William, D.Sc.
Howes, Professor George Bond, F.L.S.
Howorth, Sir Henry Hoyle, K.C.I.E.
Jones, Professor John Viriamu, M.A.
King, George.
Knobel, Edward Ball, F.R.A.S.
Lockwood, Charles Barrett, F.R.C.S.
Love, Augustus Edward Hough, M.A.
Lydekker, Richard, B.A.
Macewen, Professor William, M.D.
McConnell, James Frederick Parry, Surgeon - Major, F.R.C.P.
MacMunn, Charles, M.D.
Mansergh, James, M.Inst.C.E.
Martin, John Biddulph, M.A.
Martin, Sidney, M.D.
Matthey, Edward, F.C.S.

Mott, Frederick Walker, M.D.
Newton, Edwin Tully, F.G.S.
Notter, James Lane, Surgeon-
Lieut.-Col.
Oliver, John Ryder, Major-
General R.A.
Ord, William Miller, M.D.
Reade, Thomas Mellard, F.G.S.
Roberts, Ralph A., M.A.
Rutley, Frank, F.G.S.
Salomons, Sir David, M.A.
Sherrington, Charles Scott, M.B.
Stebbing, Rev. Thomas Roscoe
Rede, M.A.
Stevenson, Thomas, M.D.
Stewart, Professor Charles,
M.R.C.S.
Stirling, Edward C., M.D.
Thomson, Professor John Millar,
F.C.S.

Thornycroft, John Isaac, M.Inst.
C.E.
Trail, Professor James William
Helenus, M.D.
Tuke, Daniel Hack, M.D.
Ulrich, Professor George Henry
Frederick, F.G.S.
Veley, Victor Hubert, M.A.
Wallace, Alfred Russel, LL.D.
Waterhouse, James, Colonel.
Watkin, Henry Samuel Spiller,
Lieut.-Col. R.A.
Webb, Francis William, M.Inst.
C.E.
Woodward, Horace Bolingbroke,
F.G.S.
Worthington, Professor Arthur
Mason, M.A.
Young, Professor Sydney, D.Sc.

The following Papers were read :—

1. " Harmonic Analysis of Hourly Observations of Air Tem-
perature and Pressure at British Observatories. Part I.
Temperature." By Lieut.-General R. STRACHEY, R.E.,
F.R.S. Received January 20, 1893.

(Abstract.)

This paper is a discussion of the results of the computations of
the harmonic constants contained in a volume recently published by
the Meteorological Office. The tables in this volume give the con-
stants of the harmonic components of the first four orders, for each
month for twenty years, of the daily curves of temperature and pres-
sure at Greenwich ; and the constants for the first three orders, for
the temperature and pressure, for each month for twelve years, at the
seven observatories maintained by the Meteorological Office.

The tables supply the values of the coefficients of the cosines and
sines of the several terms of the usual harmonic series, representing
any hourly value :—

$$A_n = p_0 + p_1 \cos n \cdot 15° + q_1 \sin n \cdot 15° + \&c. \quad \ldots\ldots\ldots (1).$$

They also give the amplitudes of the several components, and the
epoch of maximum derived from the formula

$$A_n = p_0 + P_1 \sin (n.15° + T_1) + P_2 \sin (2n.15° + T_2), \&c. \ .. \quad (2).$$

In these tables, and the present discussion, the coefficients of the cosines of the arcs for the several components are designated by the letter p, and those of the sines by the letter q. The amplitude for the several components is designated by P, and the epoch of the first maximum that occurs after midnight is designated by the letter μ.

By the introduction of the epoch of maximum, the connexion of the component with the hour of the day and the sun's place is directly indicated, which for the purpose of these discussions is more convenient than the method usually adopted, of stating the value of the angle T in formula (2).

Reference is made to difficulties and uncertainties that occasionally arise in computing the mean values of some of these constants. Where there are periodical variations of value which lead to changes of sign, the arithmetical mean will tend to obliterate variations which may, in truth, be strongly marked. Also difficulty at times arises in respect to the epoch of maximum, from uncertainty, in dealing both with casual irregularities, and periodical changes, in saying whether the epoch has been thrown forward or backward.

The absolute *magnitudes* of the coefficients p and q indicate the amplitude of the component, and their *signs* the phase, or the epoch of maximum. It will readily be seen that the combinations of coefficients, $+p+q$; $-p+q$; $-p-q$; $+p-q$, correspond respectively to epochs of maximum in the first, second, third, and fourth quadrants of the period of the component; and the mutual destruction of a series of positive and negative values of p and q in a mean value will therefore only signify that there is no true mean epoch of maximum, and that all positions are alike probable or uncertain.

The foregoing remarks apply to the whole series of computations; what follows refers only to the temperature tables, to which the present communication is limited.

1. *Greenwich Temperature.*

The examination of the tables shows that, with very considerable variations of absolute magnitude, there is on the whole very marked consistency in the main characteristics of the components.

Taking as a test the position of the epoch of maximum, which may be regarded as more directly dependent on the sun's action, and on his position, it will be seen that the values of μ indicate very clearly the closeness of this connexion.

In all the components a truly periodical variation of the value of μ is apparent, and the period of maximum always travels *backwards*, that is, it becomes *earlier as the year passes from winter to summer*,

while it returns in the opposite direction in the change back to winter.

For the first component the variation of the five years' mean of μ from the twenty years is in no month more than $2\frac{1}{2}°$ or ten minutes of time, and the average for all months is less than half that amount.

In the second component the epoch of maximum, which during the winter months is always *after* midnight, falls back in the summer, when it is at times *before* that hour. The variation of the five-year mean from the twenty-year mean is in no month more than 6°, and the average is only 2°·3, or nine minutes of time.

In the winter months the maximum of the third component is always between 4 A.M. and 5 A.M.; in March it changes rapidly, in the summer being found invariably between midnight and 1 A.M., while after September it returns to its winter position. The variation of the five-year from the twenty-year mean in no month exceeds 5°, and the average in all months is only 2°·1, or $8\frac{1}{2}$ minutes of time.

The fourth component shows double maxima and minima, the former at the equinoxes, the latter at the solstices. The largest variation of the five-year mean of any month from the twenty-year mean is 10°, and the average for all months is 4°·3, or seventeen minutes. Considering how small are the absolute values of the coefficients p_4 and q_4, on which the value of μ_4 depends, the average being a little less than $\frac{1}{10}$th of a degree Fahrenheit, it is rather a matter of surprise that the variations should be so small than that they should reach their actual amounts.

It may be noticed that the total amplitude of the components being $\sqrt{(p^2+q^2)}$, a considerable variation of its value is quite consistent with invariable or slightly varying values of μ, which depend on the ratio p/q.

The component of the first order, which in the winter is more than double the magnitude of any of the others, and in summer more than ten times as great, gives the dominant character to the daily curves of temperature. In the series of twenty years variations in different years of as much as 100 per cent. are to be found for almost every month, but for the most part even these irregularities disappear in the mean of a series of five years, and the monthly means for the twenty years are remarkably consistent.

The progression of the value of P, in the course of the year, follows approximately the sine of the sun's meridional altitude, and the empirical formula

$$P = 10\cos z - 0.91$$

gives a close approximation to the values shown in the tables, if a "*lagging*" of eight or ten days is allowed in reckoning the sun's *place*.

The second component has two clearly marked *maxima* about the time of the equinoxes, and a principal *minimum* at midsummer.

The component of the third order varies in a converse manner, having two well-marked *minima* at the equinoxes, with a principal *maximum* at midsummer.

The component of the fourth order appears to combine the characters of the two previous ones, having two *maxima* about the time of the equinoxes, and a principal *minimum* in the winter.

The following empirical formulæ give close approximations to the values of P_2 and P_3:

$$P_2 = 1{\cdot}08 + 0{\cdot}20 \cos (\lambda + 126°) + 0{\cdot}41 \cos (2\lambda - 2°),$$

$$P_3 = 0{\cdot}42 + 0{\cdot}16 \cos (\lambda + 260°) + 0{\cdot}10 \cos (2\lambda - 172°),$$

in which λ is the sun's longitude.

The mean value of μ for the first component is 214°, corresponding to 2 h. 26 m. P.M., the variation due to season being 12° or 48 m. of time, by which the maximum is earlier in summer than in winter.

In the second order the first maximum in June is 24°, or 1 h. 20 m. earlier than in January.

In the third order the difference in the same direction is 63°, or 4 h. 12 m. of time.

In the fourth order, there is some doubt as to the manner in which the change of epoch of the summer and winter maxima is brought about. From March, when the first maximum occurs about 60° after midnight, or 4 A.M., there is a continued retrogression till June, when the maximum is at 16° after midnight, or 1 h. 4 m. A.M. This is followed by a progression from June till October, when the maximum again occurs at about 60°, or 4 A.M.

In passing from October to November, a sudden change takes place by which the maximum is established at about 10° after midnight. There is a like sudden change between January and February in the opposite direction, which again brings the maximum to 60° after midnight. From the component in November and February being very small, it is not improbable that these sudden changes may coincide with the component becoming zero.

Remembering that the fourth component includes four series of undulations, the most probable explanation of these changes is to be found in a change of the position of these undulations, during which, between January and February, the first recedes, and its place is taken by the second, which leads to sudden appearance of a maximum about 60°, or 4 A.M. A similar change between October and November in an opposite direction would introduce the maximum at 10° after midnight.

In the summer months (May, June, and July) the temperature curve during the day hours, from 8 A.M. to 8 P.M., hardly differs from

a curve of sines, the first component being more than ten times as large as any of the others, which therefore influence the temperature, relatively, very little.

The relation of the epoch of the first maximum of the component of the third order to the time of sunrise is decidedly marked, the former occurring, on the average, about 12°, or 48 m. after sunrise; the mean deviation of the interval from that amount being only 7°, or 28 m.

The periodical variation in the position of the maximum leads, during the winter months, to a *positive* maximum of this component about 1 p.m., which is combined with *negative* maxima four hours earlier and later, which correspond to the *reduced* temperature in the mornings and afternoons of the *shorter* days. In like manner, in the summer months, when this component has a negative *maximum* about 1 p.m., instead of a negative *minimum*, as in winter, there will be two *positive* maxima, one four hours earlier, the other four hours later, corresponding to the *higher* temperature in the mornings and afternoons of the *longer* days.

It will be seen that these positions of the midsummer and midwinter maximum phases correspond respectively to days of 16 hours with nights of 8 hours, or days of 8 hours and nights of 16 hours, and that at these seasons, when the variations of temperature, due to these differences, are greatest, the amplitudes of this component are also the greatest. At the equinoxes, with 12-hour days and nights, the component becomes a minimum; and at this season the change in the position of the maximum takes place as already noticed.

It might be supposed that an analogous relation between the fourth component and the occurrence of days of 18 hours, combined with nights of 6 hours, and *vice versâ*, is likely to arise. But the data are not forthcoming to test this.

Although the several components of the temperature curve cannot be regarded as indications of specific physical efficient causes, the examination of the graphical representations of the various curves presents points to which attention may usefully be drawn. The chief of these are the following :—

In the summer months the time of mean temperature is nearly where the first component becomes zero, the second and third components then balancing one another.

In the winter the time of morning mean temperature is later than in summer, and occurs when a positive value of the first component is equal to a negative value of the second.

The time of afternoon mean temperature throughout the year is somewhat either before or after 7 p.m., and almost exactly coincides *with the time* when the first and second components are equal, with *opposite signs*.

In the summer the time of absolute minimum is between the hours of 3 A.M. and 6 A.M., during which the whole of the components are negative.

Sunrise in December is about an hour and a half before the time of mean temperature; while in June it is more than four hours earlier.

Sunset in December is rather more than three hours *before* the time of mean temperature; in June it is about half an hour *after* that time.

The *rationale* of some of the empirical rules for obtaining the mean daily temperature from a limited number of observations is supplied by reference to the harmonic expressions for the hourly deviations of temperature from the mean value; it being borne in mind that the relative magnitude of the fourth component is very small.

In the first place, it will be seen that by adding together the harmonic expressions for any two hours twelve hours apart, the whole of the *odd* components disappear, and that the sum is twice the mean value, added to twice the sum of the *even* components of the selected hours, which are equal. Disregarding the components above the fourth order, if the selected hours are such that the component of the second order is zero, which will be the case at hours corresponding to $\mu_2 + 45°$ or $\mu_2 + 135°$, then half the sum of the temperatures at the selected hours will be the true daily mean added to the fourth component for the selected hour, which at English stations will never amount to $\frac{1}{2}°$, and on the average is less than $\frac{1}{4}°$.

At Greenwich the mean between the observations at $4\frac{1}{2}$ A.M. or $10\frac{1}{2}$ A.M. and the corresponding afternoon hours in January, will differ by less than $\frac{1}{16}°$ from the true value, and similar results will be obtained for June by the mean of observations made at 3 A.M. or 9 A.M. and the corresponding hours in the afternoon.

By taking the mean of observations at any four hours, at intervals of six hours, both the odd components and those of the second order will disappear, and the result will only differ from the true mean by the amount of the fourth component for the selected hours.

As this component disappears when $\mu_4 \pm 22\frac{1}{2}° = 0°$ or $180°$, the hours at Greenwich that will give the best result are 2, 8, 14, and 20, or 5, 11, 17, and 23.

So, if the mean of any three hours at equal intervals of eight hours be taken, the sums of the first, second, and fourth components will disappear, and the result will only differ from the true mean by the amount of the third component for the selected hours, which in no case can be so much as $\frac{3}{4}°$.

By adopting hours when $\mu_3 \pm 30 = 0°$ or $180°$, the third component disappears, and this result will be obtained at Greenwich by combining *observations* at *3, 11, and 19* hours, or *7, 15, and 23*.

2. *Temperature at the Seven Observatories.*

The examination of the tables will show that in their main characteristics the results closely resemble those for Greenwich, and it will not be necessary to discuss them in any detail.

The amplitude of the component of the first order is, however, in all cases less than that observed at Greenwich, the lowest values being those for Valencia and Falmouth, no doubt due to their position on the sea coast, for which stations the means for the years are 2°·28 and 2°·35 compared with 5°·10 at Greenwich.

The Kew values most resemble those at Greenwich, but the mean maximum at Kew is more than 1° less, and the mean for the year $\frac{1}{2}$° less.

The mean values of μ_1 for the seven observatories lie between 205° and 220°, that for Greenwich being 214°. The means of the summer values are about 3° or 4° less than the mean of the year, and of the winter values as much above it, as in the case of Greenwich.

The amplitudes of the first components conform approximately, but not so closely as at Greenwich, with the sine of the sun's meridian altitude, but with a flattening of the curve in the summer months, and a tendency at some of the stations to a maximum value in May.

The components of the second and third orders, beyond which the analysis is not carried for these observatories, conform in all important respects to those for Greenwich, the numerical values of the latter being, however, in all cases somewhat higher. The epochs of maximum follow the same laws, with an increased divergence of the summer epoch from that of the winter at the more northern stations.

Making allowance for their smaller amplitude, the empirical formulæ expressing the mean values of the P_2 and P_3 components differ little from those obtained for Greenwich.

In order to test, and in some degree throw light on the character and significance of the harmonic components of temperature that have been under discussion, and bearing in mind that they cannot be considered to represent separate effects of physical forces operating at the assumed periods of the components, I have, at the suggestion of Professor G. Darwin, calculated the harmonic components that would produce a curve representing an intermittent heating action such as that of the sun, continued only during a portion of the day, and commencing and ending abruptly at sunrise and sunset.

Such a procedure disregards all cooling effects, and only deals with the sun's direct heating action, which I have assumed to be *proportional to the sine of his altitude.* Also with a view of obtain-

ing figures in some degree comparable with those derived from actual observation, I have assumed (following the empirical formula before given) the power of a vertical sun to be 10. Having calculated the sun's altitude for each hour of the day, for midwinter, the equinox, and midsummer, for certain selected latitudes, the corresponding heating effects have been computed, to which the usual method of analysis having been applied, the following results are obtained (pp. 74 and 75).

These figures represent the values of the components at midnight. The signs indicate that the maximum of the first component is in all cases at 180°, or noon; of the second component the maximum in all cases it at 0°, or midnight; of the third component in all latitudes the maximum in the winter is at 60°, or 4 A.M., in the summer at 0°, or midnight, the change taking place at the equinox, when the component becomes zero; of the fourth component in the lower latitudes up to 40°, the maximum is at 45°, or 3 A.M., at all seasons; in the higher latitudes the maximum is at 0°, or midnight, in winter and summer, and at 45°, or 3 A.M., at the equinox.

For comparison, the following results from actual observations at latitudes specified are also given in similar form.

The close correspondence of the main features of these two tables is obvious.

The conclusion is unavoidable, that, although both in the actual and hypothetical cases the harmonic components when combined are truly representative of the peculiar forms of the curves from which they were derived, this affords no evidence of the existence of recurring cycles of action corresponding to the different components, but that the results are, to a great extent, due to the form of the analysis.

The diurnal curve of temperature is not symmetrical in relation to the mean value, the maximum day temperature being much more in excess than the minimum night temperature is in defect. To adjust the first component, which is symmetrical about its mean value, to the actual unsymmetrical curve, it must be modified by the other components. That of the second order which has one of its maxima not far removed from the minimum of the first order supplies the chief portion of the compensation due to this cause.

Further, from the character of the analysis, when the diurnal curve is symmetrical on either side of the hour half-way between noon and midnight, that is, when the day and night are equal in length, the third component becomes zero. Any departure from this symmetry introduces a component of the third order, with the result that with a day shorter than 12 hours one maximum will fall in the day between 6 A.M. and 6 P.M., and the other two in the night between 6 P.M. and 6 A.M.; while with *a day longer* than 12 hours, two maxima will

Latitude.	Winter.				Equinox.				Summer.			
	Components.				Components.				Components.			
	I.	II.	III.	IV.	I.	II.	III.	IV.	I.	II.	III.	IV.
0°	−4·6	+1·9	0	−0·4	−5·1	+2·1	0	−0·5	−4·6	+1·9	0	−0·4
20	−3·4	+1·7	−0·3	−0·8	−4·7	+2·0	0	−0·4	−5·3	+1·7	+0·8	−0·8
30	−2·8	+1·4	−0·3	−0·3	−4·4	+1·7	0	−0·4	−5·3	+1·6	+0·4	−0·2
40	−1·9	+1·2	−0·4	−0·1	−3·9	+1·6	0	−0·4	−5·2	+1·2	+0·4	−0·1
45	−1·5	+1·0	−0·4	+0·1	−3·5	+1·5	0	−0·4	−5·2	+1·1	+0·4	+0·1
51½	−1·0	+0·8	−0·4	+0·1	−3·2	+1·3	0	−0·3	−4·8	+0·8	+0·4	+0·2
65	0	0	0	0	−2·2	+1·0	0	−0·2	−3·9	0	+0·1	+0·1

Stations.	Summer. Components.				Equinox. Components.				Winter. Components.			
	I.	II.	III.	IV.	I.	II.	III.	IV.	I.	II	III.	IV.
Singapore .... (Lat. 1° 15′ N.)	−4·6	+1·5	+0·3	−0·5	−6·5	+2·0	+0·7	−0·5	−5·0	+1·8	−0·3	−0·2
Hong Kong .... (Lat. 22° 18′ N.)	−1·9	+0·6	+0·1	−0·1	−2·0	+0·7	+0·0	−0·1	−2·4	+1·0	−0·7	−0·8
Lyons .... (Lat. 45° 46′ N.)	−7·6	+0·8	+0·6	+0·2	−6·3	+1·5	+0·1	−0·3	−2·5	+1·0	−0·4	+0·1
Greenwich .... (Lat. 51° 30′ N.)	−7·7	+0·6	+0·6	+0·2	−4·7	+1·3	+0·2	−0·2	−1·7	+0·8	−0·3	+0·1
Fort Rae .... (Lat. 62° 40′ N.)	−6·0	+0·6	+0·2	+0·1	−7·7	+1·9	+0·4	−0·5	−1·1	+0·7	−0·3	+0·3

occur in the day and only one in the night. In the former case the negative portions of the component correspond with the reduced morning and afternoon temperatures of the short day, and in the latter the two positive phases correspond with the higher temperature of the mornings and afternoons of the longer day.

. These conclusions are in conformity with those previously indicated.

The available data are insufficient to enable us to say whether the corresponding results connected with the fourth component are as fully supported by observation as in the case of the third, but the facts so far as they go confirm this view.

It may also be pointed out that, if instead of reckoning the epochs of maximum from midnight, that nearest to noon had been adopted, it would have been seen that there is a distinct tendency for all these epochs to approach noon, affording evidence, which is perhaps hardly required, that they are all closely dependent on the passage of the sun over the meridian.

For Greenwich the results would be—

	Winter.	Equinox.	Summer.
1st component......	222°	215°	210°
2nd „	200	198	181
3rd „	194	—	(193)
4th „	190	(193)	196

In the case of the third and fourth components, the figures enclosed in brackets are epochs of minimum.

II. "The Effects of Mechanical Stress on the Electrical Resistance of Metals." By JAMES H. GRAY, M.A., B.Sc., and JAMES B. HENDERSON, B.Sc., International Exhibition Scholars, Glasgow University. Communicated by LORD KELVIN, P.R.S. Received February 10, 1893.

(Abstract.)

This investigation was begun for the purpose of obtaining an easily worked method of testing the effect of any mechanical treatment on the density and specific resistance of metals.

For alteration of density, copper, lead, and manganese copper wires were tested. The effect of stretching was always to diminish the density, the alteration being small however: for copper about ½ per cent., and for lead ¼ per cent. The effect of drawing through *holes in a steel plate* was somewhat greater, showing at first an in-

crease of 2 per cent.; and, when the drawing was continued, the density began to diminish till, after drawing from diameter 2 mm. to 1·3 mm., it showed an increase on its original value of $\frac{9}{10}$ per cent. Several other interesting results on alteration of density were obtained.

The most important part of the investigation, however, relates to the alteration of specific resistance of copper, iron, and steel wire due to stretching; and, in connexion with this, the authors wish particularly to emphasise the advantages to be gained from using the unit of specific resistance introduced by Weber, who always defined it in weight measure, that is, as the resistance of a length of the metal numerically equal to its density and section unity.

Taking the expression for the resistance R of a wire, of section w, length l, and volume specific resistance σ_v, $R = \sigma_v(l/w)$, we have $R = \sigma_v\rho(l/w\rho)$ (where ρ is the density of the wire) $= \sigma_v\rho(l^2/lw\rho) = \sigma_v\rho(l/w)l = \sigma_w(l/w)l$, w being the weight of the length l, and σ_w the weight specific resistance of the wire. We have thus obviated the necessity of making the troublesome and uncertain measurement of the section of the wire, and only require to measure the length and the length per unit of weight, both of which can be done with great accuracy. Moreover, the weight specific resistance is found to be more nearly constant than the volume unit. Also, when only a comparison of specific resistance is required, it is not even necessary to weigh the wires, but only to measure lengths, thus making it possible to detect very small changes with great ease and accuracy.

The method used was a modification of what is known as Thomson's (Lord Kelvin's) Double Bridge Method ("New Electrodynamic Balance for Resistance of Short Bars or Wires," 'Phil. Mag.,' 4th series, vol. 24, 1862). The method being a zero one, the galvanometer could be made so sensitive as to be almost unstable, so that a change of 1 in 10,000 could be easily detected.

The tests showed that the maximum permanent stretching produced a permanent alteration in the weight specific resistance of copper of 1 per cent. After the maximum stretching had been produced, it was found that there was no permanent alteration of weight specific resistance due to renewed application of stress.

In the tests of steel wire no permanent stretching was obtained, but the effect of applying weight was to cause a very small permanent decrease of specific resistance, 0·06 per cent. at first, and when additional weight was applied there was found to be an increase of 0·06 per cent. These values are very small however, compared with the temporary alteration of 1·6 per cent.

In the tests on soft iron wire the permanent alteration due to permanent stretching was found to be $\frac{1}{3}$ per cent. After the maximum stretching had been obtained stress was again applied, and it was

found, as in the copper wire, that there was no permanent alteration unless there was permanent stretching.

Hitherto the results on specific resistance have been given by all investigators on this subject, except Lord Kelvin, in volume units, but, as the alteration in density is in every case very small, the results obtained in the present investigation are in very good agreement with those of former experimenters.

The conclusions arrived at are that for practical purposes any mechanical treatment, however severe, does not affect the electrical properties of the metals tested. As contrasted with this, it is interesting to note that the smallest impurity in the metal produces a greater change than the most severe mechanical treatment. For example, an impurity of $\frac{2}{3}$ per cent. lowers the electrical conductivity by 13·5 per cent. while an impurity of $\frac{1}{4}$ per cent. lowers it as much as 30 per cent.

III. "A New Hypothesis concerning Vision." By JOHN BERRY HAYCRAFT, M.D., D.Sc. Communicated by E. A. SCHÄFER, F.R.S. Received February 16, 1893.

[Publication deferred.]

Presents, March 2, 1893.

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March 9, 1893.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and
Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks
ordered for them.

Professor Dewar made an oral statement to the effect that he
had succeeded in freezing liquefied atmospheric air into a clear trans-
parent solid. Whether this solid is a jelly of solid nitrogen con-
taining liquid oxygen, or a true ice of liquid air, in which both
oxygen and nitrogen exist in the solid form, was, however, stated to
be a question for further research.

The following Papers were read :—

I. "On the Evidences of a Submergence of Western Europe
   and of the Mediterranean Coasts at the close of the
   Glacial or so-called Post-Glacial Period, and immediately
   preceding the Neolithic or Recent Period." By JOSEPH
   PRESTWICH, D.C.L., F.R.S., F.G.S., Corr. Inst. France, &c.
   Received December 15, 1892.

(Abstract.)

In a communication made early this year (1892) to the Geological
Society, the author showed that in the South of England, besides the
superficial drift deposits of river, sea, and glacial origin, there was
yet another which could not be refered to any of these agencies, and
which he was led to conclude was the result of a submergence of not
less than 1000 feet at the close of the so-called Post-Glacial Period.
This drift, unlike the others, does not contain either fluviatile or

marine remains, nor does it exhibit any traces of glacial action. Its component materials are always derived from the adjacent hills, and none from a distance, so that they have undergone little or no wear, whilst the only organic remains are those only of land animals and of land shells. While possessing these characters in common, this drift, to which the author gives the general term of "Rubble-drift," assumes a variety of forms or phases. Another peculiarity of this drift is that it is dispersed from many centres in a manner such as would result, on the hypothesis of the late Mr. W. Hopkins, of Cambridge, from divergent currents, if a considerable area at the bottom of the sea were elevated at a given rate, and under certain depths of water.

Some forms of this drift, especially the one overlying the Raised Beaches of the Channel, have long attracted attention. The origin of that unstratified rubble has been attributed to, 1st, an excessive rainfall and great cold ; 2nd, snow and ice slides on slopes ; 3rd, waves of translation ; 4th, flood and fluviatile action during a period of great cold. The author has already stated the objections that occur to him to these several explanations, some of which no doubt meet certain of the required conditions, but none of them embrace the whole, and they all involve consequences incompatible with the other phenomena. They all also depend on agencies that involve an amount of friction and weathering which is conspicuously wanting in the Rubble-drift. There is the further objection that this drift often exhibits results due to a force of propulsion for which the suggested causes are manifestly inadequate.

The object of this memoir is to show that there is evidence of drift beds having the same origin extending over Western Europe and the coasts of the Mediterranean. In generalising phenomena so widely spread, the author has to depend to a great extent on other observations than his own. Owing to their number, only the more prominent cases have been selected, and only such particulars can be given as will serve to prove that, howsoever they may differ in detail, they all point to a common cause and agree in showing that all are explicable on the hypothesis proposed by the author, namely, of the submergence of the land concurrent with a subsequent upheaval.

France.—On the coast at Sangatte, near Cape Blanc-Nez, the Rubble-drift, which assumes the form termed "head" by Sir H. de la Beche, overlies a raised beach, the section being in almost every respect identical with that at Brighton. The rubble is derived from the adjacent Chalk and Pliocene strata, and has, as at Brighton, the appearance of having been shot over the old cliff in lenticular masses —the most massive and prolonged being the one projected on the top. This rubble contains the remains of Mammoth and some entire land shells. Near Abbeville, a very similar drift about 40 feet thick follows the slope of *the hill, but here it forms* four divisions corresponding

with the main movements of upheaval. The last is remarkable from the circumstance that the edges of the beds are turned over and reversed in the same way that the " head " over the raised beach at Portland is reversed.

Passing on to the Channel Islands, a Raised Beach surmounted by a " head " surrounds both Jersey and Guernsey, showing that in the later glacial times, as now, those islands were separated from the mainland. As the materials of the " head "—which is not a mere talus—are all of local origin, they must have been carried down by an agent acting in a quaquaversal direction from the centre of the islands, where the hills form plateaux 300 to 400 feet high, often covered by loam or loess. As there are no rivers to have originated the required flood waters, this loess cannot have had a fluviatile origin, nor, as there are no higher grounds, could it be the result of rain-wash, neither can it be the result of the disintegration of the surface rocks. It must therefore have had an origin different from that usually ascribed to the loess, and which the author attributes to the deposition of sediment from the turbid sea-waters during submergence, whilst the " head " results from the surface *débris* together with a portion of this previously deposited sediment, swept off by divergent currents during upheaval.

The Loess.—After further reference to the phenomena on the west coast of France, the author resumes the question which has given rise to much controversy, namely, that of the origin of the loess, which extends over such large tracts in Western and Central Europe. That a certain section of it within valleys is due to river floods, there can be no doubt, but there is another section, recognised as such by most Continental geologists, to which it is not possible to assign that origin. The latter is not confined to the river valleys, but is found on the dividing water-sheds and on the high plains separating the river basins. In the North of France it attains a height of 400 to 600 feet, but in the neighbourhood of Lyons it reaches to 1300 feet, whilst in the great upper valleys of the Rhine and Danube it attains to an altitude of 1500 feet, which is even exceeded further to the east. It there covers the high plains of Hungary and Southern Russia, and is by no means restricted to valleys and depressions on the surface. Various theories have been proposed to account for this wide dispersion of the loess, the two principal of which attribute its formation :—1, to a depression of Central Europe whereby the gradient of the upper valleys was greatly reduced, while no change of level occurred nearer the sea ; 2, to the advance of the great northern ice sheet, blocking the large rivers of Eastern Europe, and damming back their waters ; 3, to storm-winds acting upon disintegrated rock-surfaces. The author points out the objections to these several views, and shows that such an accumulation of silt would

necessarily be one of the consequences of the submergence he suggests —that it is such a sedimentation as would fall from the turbid waters as they slowly advanced or rested, whilst as they retreated those portions of this sediment most exposed to the effluent currents would be again swept away. As with the other phases of the rubble-drift, the organic remains of this loess are those of a land surface only.

In the South of France and inland, the author refers the Ossiferous breccias of Nice, Antibes, Cette, Pédémar, and Santenay to one phase of the Rubble-drift. At all these places, the breccia, which contains the remains of the Mammoth, Woolly Rhinoceros, and other Quaternary animals, occurs in fissures on *isolated hills*. In explanation of their presence, it has been suggested that the bones are those of animals which fell into the fissures while still open, or else that they were remains brought together by predaceous animals. But neither of these opinions can be correct, for no skeleton is found entire, no bones in place, and none of the bones have been gnawed by Carnivora. As Monsieur Gaudry also asks in discussing the facts presented by the fissure on the "Montagne de Santenay"—a flat-topped hill near Chalons-sur-Saône—" Why should so many Wolves, Bears, Horses, and Oxen have ascended a hill isolated on all sides?" The members of the Geological Society present at the *réunion* at which this remark was made seemed to agree that the animals had met their death by drowning, but in what way was left indeterminate.

Now in all these cases the fissures are in *isolated* hills with lower lands around. At Nice the hill is 132 feet high, at Antibes, 250 feet, and at Cette, which resembles on a small scale the rock of Gibraltar, the hill rises 355 feet above the sea-level. Still more formidable are the hills inland. Mont Pédémar rises to a height of 1128 feet, whilst Santenay is 1640 feet high. Among the animal remains found in the fissures are those of—

5 Carnivores	Felis Lynx Wolf Hyæna Bear	4 Ungulates	Mammoth Rhinoceros Wild Boar Horse
2 Rodents	Lagomys Hare	3 Ruminants	Ox Deer Antelope

together with *land shells* of various living species. The breccia, which is composed of sharp *angular* fragments of the *local* rocks imbedded in a matrix of red clay or loam, is generally cemented by calcite. The bones are mostly broken and splintered into innumerable sharp fragments, and evidently are not those of animals devoured by *beasts of prey; nor have they* been broken by man. It is not pos-

sible to suppose that animals of such different natures, and having such different habitats, could in life ever have herded together. Difficult as the alternative is, the author sees no other explanation of the phenomena than that of a wide-spread temporary submergence, accompanied by strong earth tremors. In such a case it is easy to conceive that as the waters gradually advanced over the low lands, the animals of the plains would naturally seek safety on the higher grounds and hills. Flying in terror, and cowed by the common danger, the Ruminants and other Herbivores, together with the Carnivores, would, as in the case of the flooding in our days of large deltas, alike seek refuge on the same safety spot. Where that spot was an isolated hill, they would, if it were not out of reach of the flood waters, eventually suffer the same fate. Subsequently the detached limbs and bones, carried, together with the surface *débris*, by the effluent currents into the open fissures, were subjected to the clashing of the rubble and the fall of large fragments of rock from the sides of the fissures, and were crushed and broken in the way they are always found. All the results noted are in accordance with the consequences that would ensue under these conditions.

The author then describes how that portion of the Rubble-drift, which was not caught in fissures or hollows, was swept down the sides of the hills during upheaval of the land. Amongst the most illustrative instances of these, is that on the slopes of Mont Genay, near Semur. This hill, which is 1430 feet high, is capped by a characteristic Oolitic bed, and it is the *débris* carried down from this bed that forms on the slopes a breccia containing similar remains to those of the fissures at Santenay, together with *land shells* and *flint flakes* of human manufacture. Another such mass was cut through by the railway to the east of Mentone, where the base of the limestone cliffs, in which are situated the noted Mentone bone-caves, are covered by a breccia in which were found the remains of *Hyæna*, *Cave Bear* and other animals, together with flints worked by Man.

Belgium.—The author recognises the Rubble-drift in the angular *débris* termed by M. Dupont " *argile à blocaux* " which partly masks, as with the Gower caves, some of the celebrated bone-caves near Dinant. It forms a thin layer between the cave deposits and a deposit of the Stone Age, thus defining clearly its geological position. It contains, when fronting the caves, the remains of *Reindeer*, *Ox*, *Horse*, &c., which has led to its being classed with the cave deposits, but the author thinks that those remains have been derived from the beds which it overlies and has partly denuded.

Gibraltar.—The Atlantic waves have left few traces of raised beaches and " head " on the western coasts of Spain and Portugal, but on the Rock of Gibraltar there are traces of several raised beaches, covered in places by local angular rubble (or *head*). This rubble extends

over the lower slopes of the Rock on both sides. On the western side it attains a thickness of 100 feet, and is projected 550 yards seaward at an angle of 9°. It is clearly not a talus, nor is it a cone of dejection. Sir Andrew Ramsay and Professor James Geikie referred its origin to two periods of severe cold and snow slides. The objection to this is the great volume of the detritus, the size of some of the blocks (some being 12 feet in diameter), and the distance to which it is projected compared to the very limited snow-collecting surface, 1400 feet in height, and the small angle of slope. The remarkable Ossiferous Fissures of Gibraltar, which are placed by the authors between the two agglomerates or breccia—referred by them to different periods—contain remains of three species of *Felis*, of *Hyæna*, *Bear*, *Rhinoceros*, *Wild Boar*, *Ibex*, *Ox*, *Horse*, *Deer*, *Hare*. The bones are, as usual, much broken and splintered, and none belonged to one entire skeleton. A human molar and some worked flint flakes were also found.

It has been suggested that the remains are those of animals that had lived and died on the Rock, and were afterwards washed into the fissures by heavy rains. But this is difficult to conceive, and besides, there is the same incompatibility in the habits and resorts of the animals thus associated as in the other fissures before mentioned. The *Hyænæ*, *Felidæ*, and *Bears* might have frequented the dens and crags of the Rock, but the *Deer*, *Bovidæ*, *Horse*, and others could only have lived in the surrounding plains, and it has not been suggested that they were carried there by the Carnivora. A great and common danger alone could have driven together the animals of the plains and of the crags and caves. As the Rock was upheaved the divergent currents swept down on both sides of the Rock the *débris* of the limestone, disintegrated by the previous long glacial cold, together with the scattered animal remains; and that the propelling force was great, and, consequently, the rise rapid, is shown by the distance to which the breccia extends from the base of the Rock. The scale is different, and the materials are different, but in all essential respects the phenomena are analogous to those presented by the "head" at Brighton and at Sangatte. There is the same restriction to local *débris* with blocks, the same absence of wear, the same traces of rude bedding, and the same occasional presence of Mammalian remains. All this points to a common origin.

Sicily.—Traces of similar phenomena are shown to exist in Sardinia, Corsica, Italy, and the coast of Dalmatia. The remarkable caves of Sicily next arrest attention from the extraordinary quantity of bones of *Hippopotami* (belonging to hundreds of individuals) which were found in connexion with them. Twenty tons of these bones were shipped from the cave of San Ciro, near Palermo, within the first six months of working, and they were so fresh that they were sent to

Marseilles "for use in the sugar factories." How could this bone breccia have been accumulated? No predaceous animals could have brought together such a collection, and, though *Hyænæ* lived at the time, they have left no traces of their presence, nor marks of their teeth, in this wonderful mass of bones. These have been classed with the contents of bone caves, but the author shows that there are objections to this. The only other suggestion made is that the bones are those of successive generations of Hippopotami which went there to die. But this is not the habit of the animal, and, besides, the bones are those of animals *of all ages* down to the fœtus, nor do the bones show any traces of weathering or of variable exposure. The author suggests an explanation founded on the local topographical features. The plain of Palermo is encircled by an amphitheatre of hills rising to the height of 2000 to 3000 feet, and presenting mural precipices towards the plain. The caves are situated near the base of this escarpment, and at San Ciro the breccia not only faces the cave, but extends to some distance in front and *on either side*. When, therefore, the island was submerged, the animals in the plain of Palermo would naturally retreat, as the waters advanced, deeper into the amphitheatre of hills until they found themselves embayed as in a seine, with promontories running out to sea on either side, and a mural precipice in front. As the area became more circumscribed the animals must have thronged together in vast multitudes, crushing into the more accessible caves, and swarming over the ground at their entrance, until overtaken by the waters and destroyed. A few of the more agile animals may have escaped, for though the remains of Deer, Ox, Bear, and Felidæ occur, they are exceedingly scarce; but the unwieldy Hippopotami perished in hundreds. As the land afterwards emerged by intermittent stages, first the rocky *débris*, and finally large blocks from the sides of the hills were hurled down, crushing and smashing the bones, which are, with few exceptions, broken into thousands of fragments. The author accounts for the numbers of Hippopotami by the fact that after the formation of the Raised Beaches there was, as he has previously shown, a considerable elevation of the coast, which, no doubt, led, as in more western Europe, to a large increase of the land area: so that the plain of Palermo may then have been of great extent.

Malta.—The drift deposits of Malta present on the whole the same general features as those of Sicily, but, owing to its peculiar population of *dwarf Elephants* with the *small Hippopotamus*, and the absence of other larger Quaternary Mammalia, the faunal remains have a distinct local colouring. They indicate that, like the Channel Islands, Malta had been long isolated before the spread of the Rubble-*drift*; but, nevertheless, it is evident that it did not escape the *catastrophe which* affected the adjacent lands. On the south side of

the island escarped rocks rise abruptly to the height of 200 to 300 feet. The lower part of these slopes is covered by a consolidated red breccia consisting of angular fragments of the local rocks, mixed with the red earth which covers the hill tops, and containing in places remains of the pigmy Elephant. The author takes this breccia to be the representative of the *head* at Brighton and Sangatte, only that in this instance the height of the escarpment has prevented its being entirely swamped, as are the old cliffs at those places. It resembles closely the breccia on the Mentone slopes. It is probable that this island, no part of which exceeds a height of 800 feet, was entirely submerged, for not a single species nor even one genus of its Quaternary Mammalia are now found living on the island, nor did any of its peculiar forms pass to the adjacent lands.

Greece.—The surface deposits of Turkey and Southern Russia are, seemingly, in general accordance with the views here expressed. The rubble beds are, however, better developed in Greece, and are there associated with an osseous breccia. This angular rubble forms great sheets extending to the shore, where they are worn back, and form cliffs 30 to 40 feet high, whilst the present torrents cut through and carry down this drift, spreading it out on the coast in the form of cones of dejection, which often becomes re-cemented like the older breccia. On the adjacent island of Cerigo ossiferous fissures, said to contain human remains, occur on the summit of an isolated flat-topped hill.

In Crete there are, in places, immense accumulations of angular detritus, and at one spot a Raised Beach is overlaid by a calcareous breccia analogous to the head of the coasts of the Channel. In the island of Rhodes is a breccia which is said not to be distinguishable from that of Greece.

Asia Minor.—M. de Tchihatchieff says that Quaternary deposits are much less common in Asia Minor than in Europe, and that there are detrital deposits of local origin on the slopes of the hills which may be Quaternary or modern, and remarks on the absence of organic remains in these superficial drifts.

A raised beach, 5 to 30 feet above present sea level, surrounds Cyprus, but it does not appear to be accompanied by a *head*, though a sandy bed, "like loess," overlies it in places. Nor is there any record of ossiferous breccia or fissures. This may be owing to the submergence here having been small.

On the coast of Palestine raised beaches range up to the height of 220 feet, but the author cannot find any record of an overlying rubble or head. Traces of a bone-breccia of uncertain relations have, however, been found near Beyrout, and detrital deposits are alluded to; but the only bone cave described appears to be of Neolithic Age. *No ossiferous fissures nor remains of Quaternary Mammalia have*

been noticed. The author concludes that the submergence of the district (if any) must have been small, but of its extension further eastward he has no means of judging. Monsieur L. Lartet states that stone implements of the Palæolithic type have been found on the surface near Bethlehem, and in some other places.

North Africa.—The coast of North Africa presents confirmatory evidence. It is fringed by raised beaches—one in particular, 10 to 40 feet above the sea-level, is very constant. Ossiferous fissures are met with on the coast at Tetuan, Oran, and other places in Algeria. They present the same characters, and contain the remains of similar animals, as those at Nice and Gibraltar. The fissures do not, however, seem to extend beyond Algeria, for none have been recorded in the province of Constantine, though there is a breccia which is suggestive of a Rubble-drift.

Eastward of Tunis, the country has been described as consisting of rolling hills of cretaceous rocks in a sea of Quaternary drift, which from the account of it closely resembles a rubble-drift, but osseous breccias and fissures seem absent. It would appear, therefore, that, as on the north shores of the Mediterranean, there was a decrease in the depth of submergence as we proceed from west to east.

Egypt.—It may in fact be a question whether the submergence extended in this direction beyond the Libyan Desert. The escarped limestone hills and long lines of quarries in Egypt show no ossiferous fissures, nor does there seem to be any Rubble-drift overlying the fluviatile terraces of the Nile, or underlying the river alluvium. Nevertheless there is reason to believe that Palæolithic Man did exist there, for flint implements of the same type as those of the Thames and Somme Valleys have been found, but they were all on the surface, and none are from any deposit of well-ascertained Quaternary age. It may further be noticed that several of the animals which disappeared with the rubble-drift in the more western districts, such as Lion, Panther, Spotted Hyæna, Hippopotamus, African Elephant, Caffir Cat, survived in the Nile Valley to historic times.

In conclusion, the author deals with certain objections which he foresees may be raised to the proposed hypothesis, especially that respecting the absence of marine remains on the submerged lands. This, however, he attributes to the short duration of the submergence, which neither allowed time for ordinary marine sedimentation, nor for the migration and establishment of a marine fauna on the submerged area, and also to the turbid condition of the waters. To the objections based on uniformitarian grounds to the rate of upheaval, he does not attach so much weight, as it seems to him that uniformity of energy in dealing with a body like the globe cannot be *admitted. The question* should be judged by the evidence of facts *and not decided by an uncertain* postulate.

The facts, on the other hand, show that all the phases of the Rubble-drift are such as may be due to the agency of a common cause. Briefly, whether it be the *head* over the raised beaches, the *osseous breccia* on slopes, or the *ossiferous fissures*, they all present a complete absence of that wear which must result from river, sea, or ice action; all the materials are of *local* origin, while all the faunal remains of these, and of one section of the *loess*, are such as might come from the wreck of a *land surface*, and a land surface only. The bones of the animals have evidently been subjected to considerable but not lasting violence, for they are *broken* and *splintered*, yet *not worn;* and though these remains are associated together in as it were a common grave, it is impossible to suppose that under the ordinary conditions of animal existence, such dissimilar orders could have been associated in life, nor, as the bones are *free from all traces of gnawing*, could those remains have been collected and left by beasts of prey. These concurrent conditions, together with the mode of distribution of the Rubble-drift from many *independent centres*, seem to the author—howsoever startling may be the conclusion—to be only explicable upon the hypothesis of a wide-spread and short submergence.

Another consequence the author draws from the position of the Rubble-drift, and one that confirms a conclusion which he had drawn from very different data, is that it affords grounds to believe that in estimating the time elapsed since the so-called Post-Glacial Period, instead of a measure of 80,000 to 100,000 years, one of 10,000 to 12,000 years would be a closer approximation. For it will have been observed that, where present, only a few feet of that peculiar drift separates the deposits of Quaternary Age from those of the newer Stone or Neolithic Age, and that nowhere have there been found between the two any sedimentary beds representing the work of any long period of time. Further, the surface configuration has remained since then comparatively unaltered.

Nevertheless, the author is fully alive to the difficulties attendant upon the hypothesis he ventures to put forward. Some of these concern naturalists rather than geologists, and the opportunity for their discussion has not hitherto arisen. He invites younger geologists to follow up the enquiry, and submits that, so far as the actual phenomena are concerned, the hypothesis satisfies, on the whole, all the more important conditions of the problem.

II. "The Electrolysis of Steam." By J. J. THOMSON, M.A.,
F.R.S., Cavendish Professor of Experimental Physics in
the University of Cambridge. Received February 18, 1893.

It is well known that steam is split up into hydrogen and oxygen
when an electric discharge passes through it. A very careful ex-
amination of the laws of this phenomenon was made more than thirty
years ago by Perrot;* as his results are very remarkable, and seem
to be not at all well known, I will describe, as briefly as possible, his
apparatus, and the results he obtained with it. The apparatus used
by Perrot in his experiments is represented in fig. 1, taken from his
paper. The spark passed between two platinum wires sealed into

Fig. 1.†
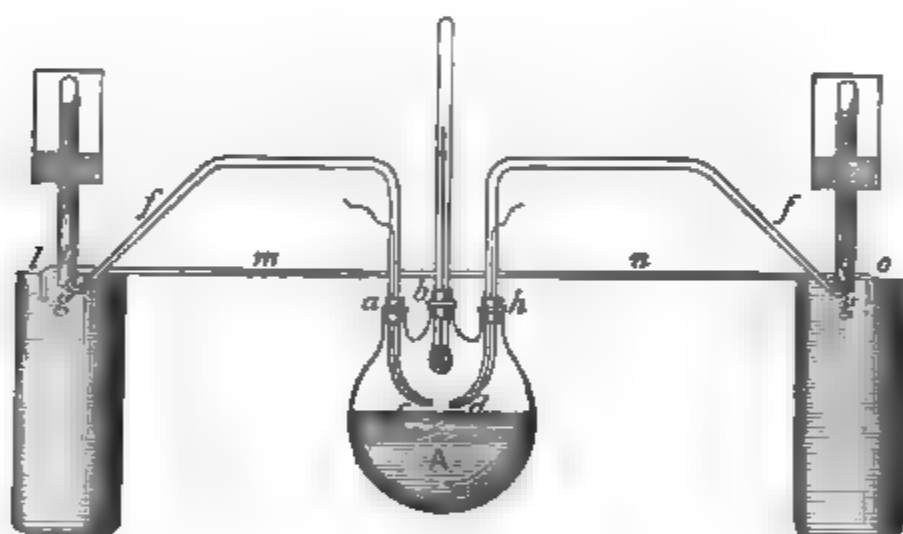
glass tubes, *cfg*, *dfg*, which they did not touch, except at the places
where they were sealed ; the open ends, *c*, *d*, of these tubes were about
2 mm. apart, and the wires terminated inside the tubes at a distance
of about 2 mm. from the ends. The other ends of these tubes were
inserted under test-tubes *e*, *e*, in which the gases, which passed up the
tubes, were collected. The air was exhausted from the vessel A, and
the water vapour through which the discharge passed was obtained
by heating the water in the vessel; special precautions were taken
to free this water from any dissolved gas. The stream of vapour
arising from this water drove up the tubes the gases produced by the
passage of the spark; part of these gases was produced along the
length of the spark. Part of the gases so collected has been
decomposed by causes which would not be affected by reversing the
electrical conditions of the electrodes, *e.g.*, by such causes as the heat

* 'Annales de Chimie et de Physique' [3], vol. 61, 1861, p. 161.
† From 'Notes on Electricity and Magnetism' (Clarendon Press, Oxford).

produced by the sparks. We should not expect to find any simple relation between the amount of decompositions from these causes and the quantity of electricity which had passed through the gas. The hydrogen and oxygen produced by such causes would, however, be driven up the tube in chemically equivalent proportions, and could therefore be eliminated by sending a spark through these gases, when they would recombine and form water.

When the sparking had ceased, the gases which had collected in the test-tubes *e* and *e* were analysed; in the first place they were exploded by sending a strong spark through them; this at once got rid of the hydrogen and oxygen which existed in chemically equivalent proportions, and thus got rid of the gas produced by heat, &c., along the length of the spark. After the explosion, the gases left in the tubes were the hydrogen or oxygen in excess, together with a small quantity of nitrogen, due to a little air which had leaked into the vessel in the course of the experiments, or which had been absorbed by the water. The results of these analyses showed that there was always an excess of oxygen in the test-tube in connexion with the positive electrode, and an excess of hydrogen in the test-tube connected with the negative electrode, and, also, that the amounts of oxygen and hydrogen in the respective tubes were very nearly chemically equivalent to the amount of copper deposited from a solution of copper sulphate in a voltameter placed in series with the discharge tube.

The results of some of Perrot's experiments are shown in the following table :—

Duration of experiment.	Weight of Cu deposited in voltameter and its equivalent in c.c. of H.	Excess of H in tube next − electrode.	Excess of O in tube next + electrode.
4·0 hours	8·5 mgm. Cu; 3·00 c.c. H	3 00 c.c.	1·40 c.c.
4·0 „	6·0 „ 2·12 „	2·10 „	0·95 „
3·0 „	5·5 „ 0·94 „	1·80 „	0·85 „
3·5 „	6·0 „ 2·12 „	2·05 „	0·90 „

Thus in Perrot's experiments the excess of hydrogen appears at the negative electrode, the excess of oxygen at the positive, and these excesses are very nearly chemically equivalent to the amount of Cu deposited in a copper sulphate voltameter placed in series with the discharge tube. Ludeking[*] confirmed the result that when sparks pass through steam there is an excess of oxygen at the positive, and of hydrogen at the negative, electrode.

As these results bear very closely on the method by which the dis-

* '*Phil. Mag.*' [5] vol. 33, 1892, p. 521.

charge passes through gases, and seem to have special reference to a view which I have long held, that the discharge through gases is accompanied by chemical changes analogous to those which take place in electrolytes conveying currents, I was anxious to repeat and, if possible, extend them. On attempting to do this, I met with very considerable difficulties, and it has taken more than a year's work to overcome these, and to arrange the experiments so as to get definite and consistent results. For this reason, as well as from the fact that my results differ very materially from those obtained by previous experimenters, I shall enter at greater length into the details of the experiments than would otherwise be necessary.

The form of apparatus which I now use, though similar in its main features to that used by Perrot, differs from it in some respects. Before describing the apparatus in detail, I will indicate the chief points of difference between it and Perrot's.

One source of doubt in Perrot's experiment seemed to me to arise from the proximity of the tubes surrounding the electrodes to the surface of the water. These tubes were narrow, and, if they got damp, the sparks, instead of passing directly through the steam, might conceivably have run from one platinum electrode to the film of moisture on the adjacent tube, then through the steam to the film of moisture on the other tube, and thence to the other electrode. If anything of this kind happened, it might be urged that, since the discharge passed through water in its passage from one terminal to the other, some of the gases collected in the tubes might have been due to the decomposition of the water and not to that of the steam. To overcome this objection, I have (1) removed the terminals to a very much greater distance from the surface of the water, and placed them in a region surrounded by a ring burner, by means of which the steam can be heated to a temperature of 140° or 150° C.; (2) I have got rid of the narrow tubes surrounding the electrodes altogether by making the tubes through which the steam escapes partly of metal, and using the metallic parts of these tubes as the electrodes.

Though I prefer this method of arranging the electrodes as being somewhat more convenient than Perrot's form of the experiment, in which the electrodes were wires surrounded by glass tubes, I have repeated the experiments described below, using wire electrodes; the results, however, were precisely the same as those obtained when the tubular electrodes were used. One great advantage of these tubular electrodes is that the quantity of metal in them is large enough to keep them quite cool during the discharge; while, when wire electrodes are used, the end of the negative terminal becomes red hot if any considerable current passes through the steam.

Instead of following Perrot's plan of removing the mixed gases *from the collecting tubes* e, e, fig. 1, and then exploding them in a

different vessel, I have collected the gases on their escape from the discharge tube in graduated eudiometers provided with platinum terminals by means of which the mixed gases were exploded *in situ* at short intervals during the course of the experiments. This plan avoids the trouble, waste of time, and risk of error incurred in moving the mixed gases from the collecting tube to the eudiometer. Its greatest advantage, however, is that it enables us to see with very little delay at which terminals the excesses of hydrogen and oxygen are appearing. As I shall have to explain below, the sides at which the excesses of hydrogen and oxygen appear can be reversed by altering the character of the spark, and it very much facilitates the investigation of the laws of this reversal to be able to tell, with as little loss of time as possible, at which terminal the excess of hydrogen is appearing.

Description of the Apparatus.

I will now pass on to describe the form of apparatus which, after many trials, was found to be the most convenient.

This is represented in fig 2. H is a glass bulb, 1·5 to 2 litres in volume, containing the water which supplies the steam. A tube, L, about 0·75 cm. in diameter and 35 cm. long, is joined on to this. In many cases this was *fused* directly on to the bulb; I do not think, however, that this is necessary, and I have found no ill effects arise from

FIG. 2.
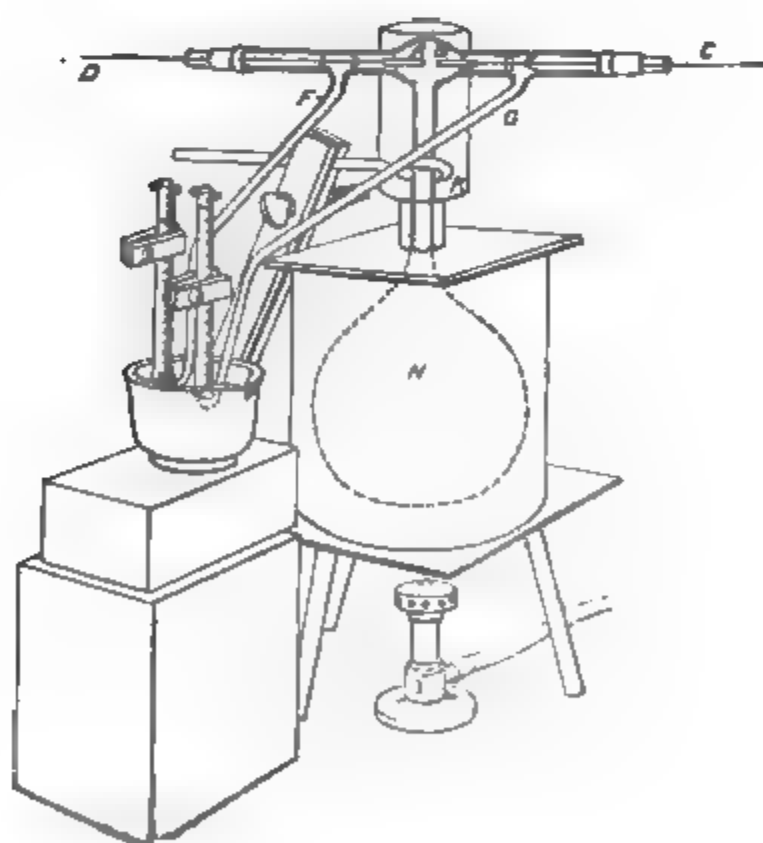
making this connexion by placing a rubber stopper in the prolonga-
tion of the bulb, and pushing the tube L through a hole in this
stopper, care being taken to push the tube right through the hole. I
may remark in passing that it is very desirable to adopt a form of
apparatus which is easily constructed; the life of these tubes is by
no means long, as they are exceedingly liable to crack, especially when
cooling, at the end of an experiment. The apparatus I am now
describing is one which was designed with special reference to easy
construction, and fused joints are done away with in those places
where I have found by experiment that this course could be taken
without injury to the accuracy of the experiments. I have, however,
repeated the experiments, using apparatus in which all the joints were
fused.

The top of the tube L is fused on to the horizontal discharge
tube CD; this tube is blown out into a bulb in the region where the
sparks pass, so that when long sparks are used they may not fly to
the sides of the tube. The top of the tube L, near its junction with
CD, is encircled by a ring burner K, and this part of the tube is
surrounded by an asbestos case; by these means the steam may be
superheated to a temperature of 140° to 150° C.

The details of the electrodes between which the sparks pass are
shown in fig. 3. For the metal parts a, b, it is necessary to use some
metal which is not oxidised by the steam, as a very small amount of
oxidation would be sufficient to render the results nugatory.

FIG. 3.
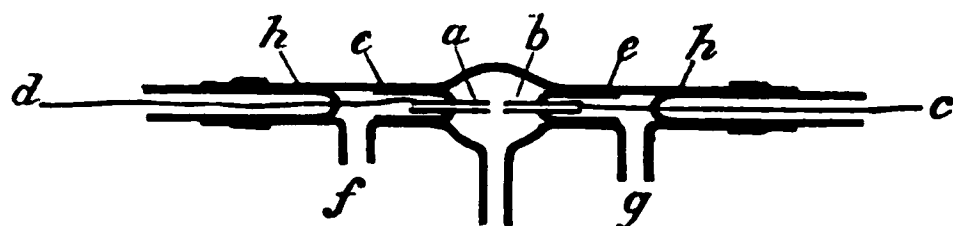
I have used as the electrodes, (a) brass tubes thickly coated with gold
with their sparking ends carefully rounded off, or (b) tubes made by
winding thick platinum wire up into a coil. These tubes are
placed in pieces of glass tubing, e, e, to hold them in position. To
facilitate the expulsion of air from the apparatus, it is desirable that
the metal tubes should not fit so tightly into the glass ones as to pre-
vent the steam from passing between the two. If they fit too tightly
to allow this, air is apt to lodge between the metal and glass tubes,
and if this gets driven out into the delivery tubes F, G (fig. 2) when
the sparks pass, it will vitiate the experiment.

The glass tubes e, e stop short of the places f, g, where the delivery
tubes join the discharge tube. The discharge tube is closed at the
ends by two pieces of tube, h, h, which have their ends inside the tube

fused up; wires connected to the electrodes c, d are fused through the closed ends of these tubes. It is desirable that the closed ends of the tubes h, h should come up as close as possible to the exits f, g, as air is very apt to remain in the tube if there are any places through which the steam does not rush. The tubes h, h may either be fused on to the spark tube or fastened to it by rubber tubing.

The delivery tubes F, G (fig. 2) are fused on to the discharge tube at f, g (fig. 3). These tubes are about 5 cm. in diameter and terminate in narrow openings. It is essential that the steam and the mixed gases should escape through the tubes F, G at approximately the same rate; to ensure this, the narrow extremities of these tubes should be equal both in length and width. This was attained by drawing out a piece of tubing which was originally of the same diameter as F, G, and then cutting it at the middle of the narrow part; the two halves were then either fused or fastened by rubber tubing to F, G. The narrow ends of F, G are turned up and placed under mercury in the vessel M (fig. 2). Over these ends, graduated eudiometer tubes are placed; these are filled with mercury at the beginning of the experiment, but the mercury soon gets displaced by the water produced by the condensation of the steam rushing through the tubes.

The heat produced by this condensation serves a useful purpose; it raises the temperature of the water in the eudiometer tubes over which the gases are collected to over 80° C., and thus, since hot water absorbs oxygen but not hydrogen much less readily than cold, diminishes the disturbing effect due to the greater absorption of the oxygen than of the hydrogen by the water over which the gases are collected.

The effect produced by electrification on the condensation of a jet of steam is shown in a very striking way by this apparatus. When the delivery tubes are open to the air, the steam, after escaping from the nozzles, goes some inches before it condenses sufficiently to form a cloud; as soon, however, as the coil is turned on and the sparks pass, brownish clouds reaching right down to the nozzles are at once formed. The cloud is denser in the steam which has gone past the negative electrode than in that which has gone past the positive.

Precautions which it is necessary to take to ensure Correct Results.

These can, perhaps, best be realised by considering that what we have to measure is the excess of hydrogen or oxygen, as the case may be, left after exploding the mixed gases. Now, if we consider, firstly, that this excess is a small fraction of the original volume of the mixed gases—the exact proportion between the two varies greatly with the length of the spark, but in some cases the excess did not amount to more than 5 per cent. of the mixed gases; secondly, that only a very small portion of the steam passing through the

discharge tube is decomposed by the spark, so that the volume of the mixed gases bears a very small proportion to that of the steam—certainly nothing like 1 : 100 in my experiments—it is evident that if the steam contains anything like 1/10 per cent. of air, the oxygen in this air will be comparable with that produced by the sparking, and its presence will prevent any reliable results being obtained.

This air may come from two sources—(1) it may be present in the tube originally; and (2) it may have been absorbed by the water.

To get rid of the air from the first source, the tube was so constructed that there were no blind alleys; every part of it was a thoroughfare for the steam. In addition to this, the vessel H (fig. 2) was, at the beginning of the experiment, filled so full of distilled water, by dipping one of the delivery tubes under the water and connecting the other to a water pump, that when the water was heated its expansion was sufficient to cause it to fill the whole of the tube and overflow.

To get rid of the air dissolved in the water, I found no plan so efficacious as prolonged boiling. In the earlier experiments, in addition to the boiling, I tried to absorb the oxygen by mixing oxidising agents with the water; finally, however, I dispensed with these and trusted entirely to the boiling to remove the air.

The distilled water was boiled vigorously for six or seven hours with the ends of the tubes F, G open to the atmosphere. The eudiometer tubes filled with mercury were then placed over the ends of the delivery tubes F, G, so that, if any air were mixed with the steam, it would be collected in these tubes. The steam was then allowed to run into the eudiometer tubes for about an hour, when the tubes were examined to see if they contained air. If any air was observed, the eudiometer tubes were removed and vigorous boiling maintained until, on repeating the experiment, the air was found to have disappeared.

As the excess of hydrogen obtained in the hour by sparking through the steam would have been at least 1 c.c., and in many experiments much more, while the air did not form a bubble large enough to be visible, we may, I think, conclude that there was not enough air present to affect the result appreciably.

Method of Producing the Sparks.

The sparks were produced by means of a large induction coil, which would give sparks about 5 cm. long when the current from five large storage cells, which was the usual battery power employed, was sent through it. The break used was, generally, the ordinary electro-magnetic break supplied with these coils, but in some experiments a slow mercury break was employed.

On trying to use the coil in the ordinary way, the current obtained was exceedingly small, so small that the hydrogen liberated in a water voltameter placed in series with the discharge tube only amounted to about 0·25 c.c. per hour. As it is inconvenient to work with such small currents, on account of the time which has to elapse before a quantity of gas can be obtained sufficient to enable accurate measurements to be taken, I endeavoured to increase the current from the coil. I found that, as I believe is the case with all induction coils, the condenser supplied with it had not nearly enough capacity to enable the coil to give out its maximum current. When I added to this condenser a large paraffin paper one, with a capacity of about 6 micro-farads, the current from the coil was increased more than twenty times, and I found no difficulty in getting from 4 to 6 c.c. of hydrogen liberated per hour in the water voltameter in series with the discharge tube.

In order to measure the quantity of electricity which passes through the spark tube, a well-insulated water voltameter was placed in series with it, and the quantity of hydrogen liberated in this voltameter observed. The gases liberated in this voltameter were repeatedly tested, in order to see whether there was any mixing up of the hydrogen and oxygen in the collecting tubes over its electrodes. Such admixture seemed possible, as the electromotive force produced when the circuit is " made " is in the opposite direction to that produced when it is broken. The test consisted in vigorously sparking through the gases collected in the voltameter, but no contraction occurred. As, however, it is very difficult to get a mixture of hydrogen and oxygen to explode if the hydrogen is greatly in excess, I added to the hydrogen in the voltameter enough oxygen to cause an explosion; the contraction in this case corresponded to the oxygen added, showing that there was no oxygen originally present. We may therefore conclude that the current sent through the secondary circuit on " making " the coil is in this case too small, in comparison with that produced on " breaking " the circuit, for its effects to be appreciable.

Method of Making the Experiments.

After it had been ascertained, in the way previously described, that all the air had been expelled from the vessel, the eudiometer tubes were filled with mercury and placed over the ends of the delivery tubes, and the spark tube connected up with the coil.

The next step was to see if the rates of flow through the delivery tubes were approximately equal. This was done by turning on the coil and collecting the mixed gases in the eudiometer tubes; if the volume of these gases in the two tubes was not the same, the appa-

ratus had to be readjusted. When everything was fused together, and there were no flexible joints, this had to be done by letting the delivery tubes F, G dip into separate basins filled with mercury, and then to raise or lower the level of the mercury in one or other of these basins until the rates of flow of the gases into the two eudiometer tubes were approximately equal.

It is not, however, necessary to have two vessels if the narrow portions of the exit tubes are connected with the main portions by flexible rubber joints turned under the surface of the mercury, as in this case it is very easy to raise or lower the end of one or other of the tubes without interfering with the rest of the apparatus.

These flexible connexions do not, as I have found by direct experiment, introduce any source of error, and add greatly to the longevity of the tube. When everything is fused up and the connexions are rigid, the shocks due to the explosion of the mixed gases are exceedingly liable to break the exit tubes from off the main tubes, while they are comparatively harmless when there is a flexible connexion between the piece of the exit tube immediately under the collecting tube and the rest of the apparatus.

When the exit tubes had been adjusted so that the rates of flow through the two tubes were the same, the mixed gases were emptied out of the collecting tubes, which were refilled with mercury; the water voltameter was placed in series with the steam tube, and the coil again set in action.

The steam which came up the collecting tubes condensed into hot water which soon displaced the mercury; the mixed gases collected over this hot water, and were exploded at short intervals of time by sparks from a small Wimshurst machine. The gases did not disappear entirely when the sparks passed; a small fraction of the volume remained over after each explosion, and the volume which remained was greater in one tube than in the other.

The residual gas which had the largest volume was found on analysis to be hydrogen; the other was oxygen. Thus, by comparing the volumes of the residual gases in the two tubes it could readily be ascertained next to which electrodes the excesses of hydrogen and oxygen were appearing.

There are other differences in the behaviour of the gases in the two tubes which, though less obvious than the difference in volume, are quite as characteristic. One of these is the difference in the ease with which explosions take place; the gases explode much more readily on the side at which the oxygen is in excess than on the other. Another very characteristic difference is that when sparks pass in rapid succession through the tube in which the oxygen is in excess bright spangles often appear floating about in the gas, due, I *imagine,* to the ignition of small pieces of platinum torn from the

electrodes. I have never observed these spangles on the hydrogen side.

When a sufficient quantity of the residual gas had been collected, which generally happened when the sparks had been passing for an hour or an hour and a half, a considerable volume of the mixed gases was allowed to accumulate, so as to make sure of an explosion when the spark from a Wimshurst passed through them. The coil was then stopped and the mixed gases exploded, and the quantity of hydrogen in the water voltameter determined.

The residual gases in the collecting tubes were then analysed; the first step was to cool these gases, which while the steam had been rushing into the tubes had been at a temperature of more than 80° C., down to the temperature of the room. After this had been done, the nature of these gases was determined by adding known volumes of oxygen and hydrogen, prepared electrolytically from water, and observing the contraction which took place when a spark passed; the addition of the hydrogen or the oxygen, as the case might be, was continued until no further contraction took place on sparking.

The result of these analyses was that when the sparks were not too long the residual gas in one tube was found to be pure hydrogen, that in the other pure oxygen; if any other gases were present their volume was too small to be determined by my analysis. This result was only attained after considerable experience with the experiments and with the precautions necessary to obtain correct results; in the earlier experiments there was always a considerable quantity of some other gas (which, I suppose, was nitrogen) present.

When the sparks passing through the steam were very long I never succeeded in getting rid of this nitrogen; indeed, in some cases it amounted to more than 30 per cent. of the oxygen. I am not sure what the source of this nitrogen is; it may have been absorbed by the electrodes and given out when the sparks pass, or it may have come from the walls of the discharge tube, as these long sparks have a tendency to occasionally jump to the walls, and when they do so they may liberate air which would otherwise adhere to the glass.

Results.

The results obtained by the preceding method varied greatly in their character with the length of the spark; I shall therefore consider them under the heads " short sparks," " medium sparks," and " long sparks."

The lengths at which a spark changes from " short " to " medium," and then again to " long," depend on the intensity of the current passing through the steam, and therefore upon the size of the induction coil and the battery power used to drive it. The limits of

" short," " medium," and " long sparks " given below must therefore be understood to have reference to the particular coil and current used in these experiments. With a larger coil and current these limits would expand; with a smaller one they would contract.

Short Sparks.

I shall begin by describing the experiments with short sparks, *i.e.*, sparks from 1·5 to 4 mm. long. Here the appearance of the spark shows all the characteristics of the " arc " discharge.

The discharge passes as a thickish column with ill-defined edges, and when placed in a wind it is blown out to a broad flame-like appearance.

For these short sparks or " arcs," as I prefer to call them, two very important laws were found to be true—

1. That within the limit of error of the experiments the volumes of the excesses of hydrogen in the one tube, and of oxygen in the other, which remain after the explosion of the mixed gases, are, respectively, equal to the volumes of the hydrogen and oxygen liberated in the water voltameter placed in series with the steam tube.

2. The excess of hydrogen appears in the tube which is in connexion with the *positive* electrode, the excess of oxygen in the tube which is in connexion with the *negative* electrode.

The second of these results surprised me very much when I first observed it, as both Perrot and Ludeking had found that in the electrolysis of steam, as in that of water, the excess of hydrogen was at the negative and that of oxygen at the positive electrode. According to my experiments, however, the electrode at which the hydrogen appears in the electrolysis of steam is of the opposite sign to that at which it appears in the electrolysis of water. So that, if a water voltameter is placed in series with the steam tube, the gases are liberated in the way shown in the accompanying diagram (fig. 4), in

FIG. 4.
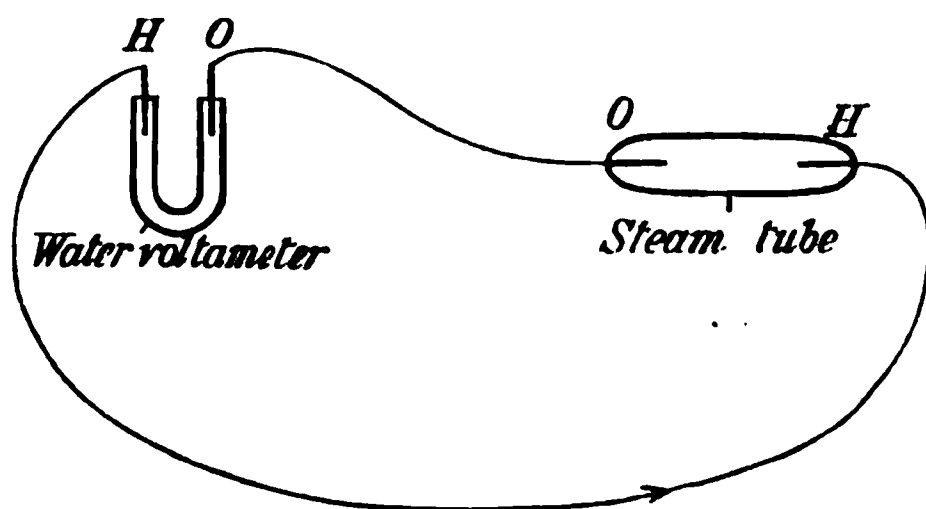
which it will be seen that the electrodes at which the hydrogen appears are next each other, as are also those at which the oxygen appears, instead of being arranged alternately as they would have been if two water voltameters had been placed in series.

A very large number of experiments were made to test the truth of this law, platinum as well as gold electrodes were tried, and I replaced the metal tubes which serve as the electrodes in my apparatus by wires surrounded by glass tubes; so as to make the form of the apparatus approximate as closely as possible to that used by Perrot; the results, however, were all perfectly definite and uniform; the hydrogen always appeared in the tube next the positive electrode, the oxygen in the one next the negative electrode.

A good method of showing the way in which the hydrogen follows the positive electrode is to keep the coil on one way until about 2 c.c. of hydrogen have been collected in the tube next the positive and about 1 c.c. of oxygen in the tube next the negative electrode, then reverse the coil; the volume of residual gas will be found gradually to diminish as the mixed gases are exploded, and this diminution goes on until the hydrogen and oxygen previously collected have quite disappeared, and the water reaches right to the top of the collecting tubes. After this, if the sparking is continued, oxygen begins to appear where hydrogen had previously been, and *vice versâ*.

The reversal of the coil supplies a very useful means of telling whether the apparatus is properly adjusted. If the rates of flow through the two delivery tubes are very different, hydrogen may appear in one of the tubes, and oxygen in the other, from some cause which is not electrical; this can be detected at once by reversing the coil.

The following table (p. 102) contains the results of some measurements of the relation between the excesses of hydrogen and oxygen in the collecting tubes attached to the steam tubes and the quantity of hydrogen liberated in a water voltameter placed in series with the discharge tube. The ordinary vibrating break supplied with induction coils was used, except when the nature of the break is indicated.

The results tabulated above show that the excesses of hydrogen and oxygen are approximately equal to the quantities of hydrogen and oxygen liberated in the voltameter.

Medium Sparks.

When the spark length is greater than 4 mm., the first of the preceding results ceases to hold. The second of these, that the hydrogen comes off at the positive electrode, remains true until the sparks are some 11 *mm. long; but, instead* of the hydrogen from the steam being

Spark length in millimetres.	Metal used for electrodes.	Excess of H in tube next + electrode.	Excess of O in tube next − electrode.	H in water voltameter.	Duration of experiment in minutes.
		c.c.	c.c.	c.c.	
1·5	gold	3·25	1·5	3·2	40
1·5	platinum	2·8	1·6	3·0	30
1·5	gold	1·7	0·8	1·8	20
2·0	gold	2·0	1·08	1·95	30
2·0	gold	3·25	1·75	3·2	60
2·0	platinum	1·8	tube broken	2·0	not noted
2·0	platinum	3·0	1·5	3·0	60
2·0.	gold	2·5	1·5	3·0	60
3·0	gold	1·8	not noted	1·8	not noted
3·0*	gold	0·7	0·4	0·8	90
3·0†	gold	1·6	not noted	1·75	not noted
4·0	gold	0·9	0·37	0·7	20
4·0	gold	2·75	1·25	2·7	60
4·0†	gold	1·0	not noted	1·25	not noted
4·0	gold	2·5	1·25	2·3	45

equal to that from the water, it is, when the increase in the spark length is not too large, considerably greater.

The following are a few instances of this :—

Spark length.	Hydrogen from steam.	Hydrogen from voltameter.
5 mm.	1·8	1·2
5 „	3·75	3·0
5 „	4·4	2·1
6 „	4·0	1·6
7 „	4·25	3·0
7 „	3·75	2·0
8 „	3·75	2·6

This increase in the ratio of the hydrogen from the steam to that from the voltameter does not continue when the length of the spark is still further increased. When the spark length has got to 8 mm. this ratio begins to fall off very rapidly as the spark length increases, and we soon reach a spark length at which it seems almost a matter of chance whether hydrogen or oxygen appears in the collecting tube connected with the positive electrode.

When the sparks are at this critical length the kind of thing which happens is somewhat as follows :—For some time an excess of hydrogen (say) comes off in the tube next the positive electrode, and accumulates as the mixed gases are exploded; then some slight

* In this experiment a slow mercury break making about four breaks per second was used.

† In these experiments large Leyden jars were attached to the electrodes.

change takes place in the action of the coil, and an excess of oxygen begins to appear; this gradually wipes out the accumulation of hydrogen, and if it goes on long enough makes the residual gas in the tube entirely disappear; then the oxygen begins to accumulate, only, however, to be wiped out later, when another change in the action of the coil has caused hydrogen to appear in excess in this tube. Thus, in this case, the residual gas in the tube does not, as before, steadily increase with the time of sparking, but is continually waxing and waning, sometimes being oxygen and sometimes hydrogen.

Long Sparks.

When the spark length is increased beyond the critical value the excess of hydrogen, instead of appearing as with shorter sparks at the positive electrode, changes over to the *negative;* the excess of oxygen at the same time going over from the negative to the positive electrode. Thus the gases when the spark length is greater than its critical value, appear at the same terminals as they do when released from an ordinary electrolyte, instead of at the opposite terminals, as they do when the sparks are shorter.

The length of spark at which this reversal takes place depends, to a very great extent, upon the current sent through the steam: the smaller the current the shorter the critical spark length. By diminishing the current by inserting a liquid resistance I reduced, on one occasion, the critical spark length from 11 to 8 mm. This critical length, too, seems to depend upon a number of small differences not easily specified; it will even vary greatly in the course of one afternoon, though apparently nothing has been changed. I have found, however, that this capriciousness disappears either altogether, or to a very great extent, if Leyden jars—very small ones will do—are attached to the terminals of the steam tube, or if an air break is placed in series with that tube. Under these circumstances the critical length will, if the same coil is used, remain constant from day to day.

It will be noticed that my results, when the length of the spark is greater than the critical length, agree with those obtained by Perrot and Ludeking, as these observers found that the hydrogen appeared at the negative, the oxygen at the positive, electrode. Ludeking worked with long sparks only, so that his results are quite in accordance with mine. In Perrot's experiments the spark length was about 6 mm. I have never been able to reduce the critical length quite as low as this, even though I diminished the current to the magnitude of that used by Perrot; I have, however, got it as low as 8 mm., and it is probable that the critical length may not be governed entirely by the *current.*

I was not able to detect any change in the appearance of the spark as the spark length passed through the critical value. My observation on the connexion between the appearance of the discharge and the electrode at which the excess of hydrogen appears may be summed up in the statement that when the discharge is plainly an arc the hydrogen appears at the positive electrode, and when the hydrogen appears at the negative electrode the discharge shows all the characteristics of a spark. However, before the spark length reaches its critical value the discharge looks much more like a spark than an arc.

With regard to the quantity of hydrogen liberated from the steam in comparison with that set free in the voltameter, I find that when the spark length is a few millimetres greater than the critical length the amount of hydrogen from the steam is very approximately the same as that in the voltameter. The following table contains a few measurements on this point :—

Spark length.	Hydrogen from steam.	Hydrogen from voltameter.
10 mm.	0·7 c.c.	0·8 c.c.
12* „	0·75 „	0·9 „
14 „	0·8 „	1·1 „

When the sparks are longer than 14 mm. the amount of hydrogen from the steam was no longer equal to that from the voltameter. The results, however, were irregular, and, as mentioned before, there was a considerable quantity of nitrogen (?) mixed with the hydrogen and oxygen.

When the sparks are very much longer, say about 22 mm., the electrode at which the hydrogen appears reverses again, i.e., the hydrogen comes off at the positive electrode, just as it does when the sparks are very short. With these very long sparks the current is extremely small, and it takes several hours to liberate 1 c.c. of hydrogen in the voltameter.

The proportion of hydrogen from the steam to that from the voltameter was with these long sparks too irregular to admit of any conclusions being drawn.

The preceding results show that in the electrolysis of steam, as in that of water, there is a very close connexion between the amounts of hydrogen and oxygen liberated at the electrodes and the quantity of electricity which has passed through the steam, and that this relation for certain lengths of sparks is the same in steam as in electrolytes. There is, however, this remarkable difference between the electrolysis of steam and that of water, that whereas in the case of

* In this experiment there was an air break 9 mm. long in series with the steam tube.

water the hydrogen always comes off at the negative, the oxygen at the positive, electrode, in the case of steam the hydrogen and oxygen come off sometime at one terminal, sometimes at the other, according to the nature of the spark.

The result obtained with the arc discharge, viz., that the oxygen appears at the negative electrode, the hydrogen at the positive, is what would happen if the oxygen in the arc had a positive charge, the hydrogen a negative one. As this is contrary to the commonly received views of the electro-chemical properties of these gases, I endeavoured to see if I could obtain any other indications of this peculiarity. With this object I have made a series of experiments on the properties of various gases when the arc discharge passes through them. I began these merely with the idea of elucidating the particular point mentioned above, but one experiment has led on to another until they form a series too long to be described here; I shall, therefore, limit myself to those experiments which seem to have the most direct bearing on the electrolysis of steam.

The apparatus used for these experiments is represented in the figure. The arc discharge between the platinum terminals A, B was produced by a large transformer, belonging to the Cavendish Labora-

FIG. 5.
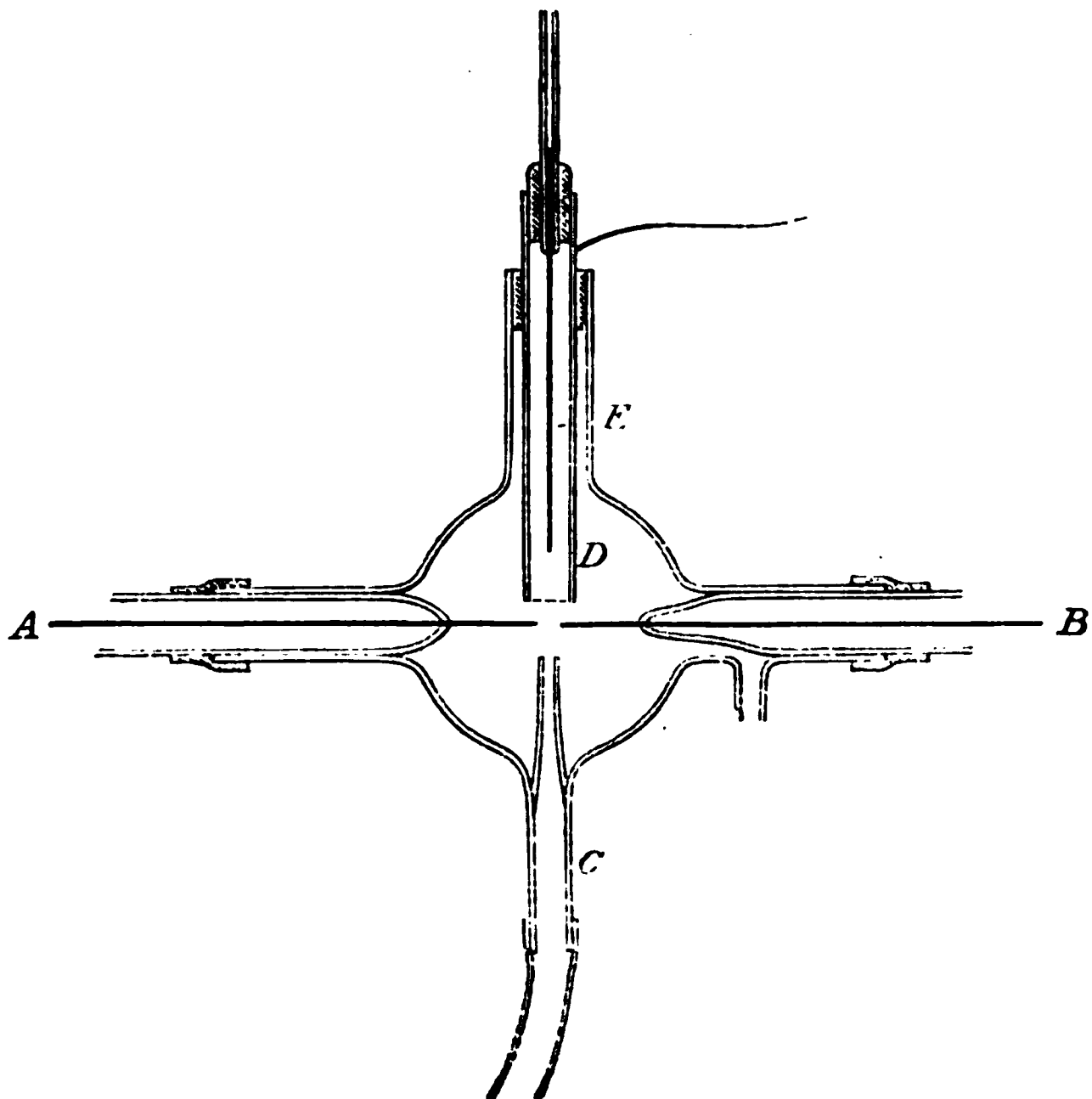
tory, which transforms up in the ratio of 400 to 1; an alternating current of about 35 ampères, making 80 alternations per second, was sent through the primary of this. A current of the gas under examination entered the discharge tube through the tube C, placed underneath the arc, and blew the gas in the neighbourhood of the arc against the platinum electrode E, which was connected to one quadrant of an electrometer, the other quadrant of which was connected to earth. To screen the electrode E from external electrical influence, it was enclosed in a platinum tube, D, the end of which was made of fine platinum wire gauze, which, though it served as a screen for electrostatic action, yet allowed the gases in the neighbourhood of the arc to pass through it. This tube was connected to earth. After passing out of the tube, the electrode E was attached to one end of a gutta-percha-covered wire wound round with tin-foil connected to the earth; the other end of this wire was connected to the electrometer.

The experiments were of the following kind :—The quadrants of the electrometer were charged up by a battery; the connexion with the battery was then broken, and the rate of leak observed. When the arc was not passing the insulation was practically perfect, the spot of light reflected from the mirror of the electrometer hardly moving appreciably in the course of three minutes. As soon, however, as the arc was started, and for as long as it continued, the insulation of the gas surrounding E in many cases completely gave way. There were, however, some remarkable exceptions to this, which we now proceed to consider.

Oxygen.

We shall begin by considering the case when a well-developed arc passes through oxygen :—

1. When the electrode E is charged negatively. In this case it loses its charge very rapidly, it does not, however, remain uncharged, but acquires a positive charge, increasing until the electrode E has acquired a potential V. V depends greatly on the size of the arc and the proximity of the electrode; in many of my experiments it was from 10 to 12 volts.

2. When the electrode is charged positively. If the potential is very high, the electrode leaks until the potential sinks to V; after reaching this potential the leak stops, and the gas seems to insulate as well as when no discharge is passing through it. If the potential to which E is initially raised is less than V (a particular case being when the electrode is entirely without charge to begin with), the positive charge increases until the potential of E rises to V.

Thus we see (1) that an electrode immersed in the arc oxygen can

insulate a small positive charge perfectly, while it instantly loses a negative one; (2) that an uncharged electrode immersed in this gas acquires a positive charge.

If the spark length is increased until the discharge passes as a spark, then the electrode leaked slowly, whether charged positively or negatively; the leak in this case is, however, very small compared to that which exists when the discharge passes as an arc.

Hydrogen.

When similar experiments are tried in hydrogen, the results are quite different.

When the *arc* discharge passes through hydrogen the electrode E always leaks when it is charged positively; it does not merely lose its positive charge, but acquires a negative one, its potential falling to U, where U is a quantity that depends on the size of the arc, and on its proximity to the electrode; in my experiments 5 or 6 volts was a common value for U. If the electrode E is initially uncharged, it acquires a negative charge, the potential falling to U, while if it is initially charged negatively it leaks if the negative potential is greater than U, until the negative potential falls to U, when no further leak occurs; if the negative potential is less than U, the negative charge on the electrode increases until the potential becomes equal to U, when it remains steady.

It is much more difficult to get a good arc in hydrogen than in oxygen, and, as it is essential to the success of the preceding experiments that the discharge should pass as a well-developed arc, the experiments with hydrogen are a little more troublesome than those with oxygen.

These experiments show that the oxygen in or near the arc discharges a negatively electrified body, but not a positively electrified one, while the hydrogen in or near the arc discharges a positively electrified body, but not a negatively electrified one. And also that an unchanged electrode becomes positively electrified in the oxygen, negatively electrified in the hydrogen.

I next endeavoured to see if this charging up of the electrodes is due to an electrification developed by the contact of the gas in the arc with the electrode, or whether this gas behaved as if it possessed an independent charge of electricity.

If the electrification is due to the contact of the gas with the electrode, then it ought to disappear when the electrode is covered with a layer of a non-conductor; if, however, the gas in the arc behaves as if it were charged, then, even though the electrode is covered with a non-conductor, the electrostatic induction due to the charge on the gas ought to produce a deflection of the electrometer in the same *direction as if the electrode were* uncovered.

I tried, therefore, the effect of covering the electrode with glass, with mica, with ebonite, and sulphur. I found that, in all these cases, the electrometer was deflected as long as the arc existed, and that the deflection was in the direction corresponding to a positive charge when the arc was in oxygen, and in that corresponding to a negative one when the arc was in hydrogen. The deflection, though not so large as when the electrode was bare, was quite unmistakable. It disappeared almost entirely as soon as the arc stopped.

Another experiment which I tried was to surround the arc by a large glass tube, coated inside and out with a thin layer of sulphur to prevent conduction over its surface. A ring of tin-foil was placed outside the tube, so as to surround the place where the arc passed; this ring was connected with one of the quadrants of an electrometer. As a further precaution against the creeping of the electricity over the surface of the tube, two thin rings of tin-foil, connected to the earth, were placed round the ends of the tube. In this case, when the arc passed through oxygen the quadrants of the electrometer connected with the central ring of tin-foil were *positively* charged by induction, while when the arc passed through hydrogen these quadrants were negatively charged. These experiments show that the oxygen in the arc behaves as if it had a charge of positive electricity, while the hydrogen in the arc behaves as if it had a charge of negative electricity.

The electrodes in the preceding experiments were so large that they were not heated sufficiently by the arc discharge to become luminous.

Elster and Geitel found that a metal plate placed near a red-hot platinum wire became positively electrified if the plate and the wire were surrounded by oxygen, negatively electrified if they were surrounded by hydrogen. If we suppose that the effect of the hot wire is to make the surrounding gas in a condition resembling the gas in the arc, Elster and Geitel's results would be explained by the preceding experiments, for these have shown that when this gas is oxygen it is positively electrified, and when hydrogen negatively electrified.

The following explanation of the results of the experiments on the electrolysis of steam seems to be that which agrees best with the preceding investigation.

When an electric discharge passes through a gas the properties of the gas in the neighbourhood of the line of discharge are modified. Thus, as Hittorf and Schuster have shown, the gas in the neighbourhood of the discharge is no longer an insulator, but can transmit a current under a very small potential difference. Faraday's remark, that when once a spark has passed through a gas the passage of another following it immediately afterwards is very much facilitated, *is another example of the* same thing. We have thus good reasons

for believing that when a spark passes through a gas it produces a supply of a modification of the gas, whose conductivity is enormously greater than that of the original gas. I have shown ('Phil. Mag.,' November, 1891) that the conductivity of this modified gas is comparable with that of strong solutions of electrolytes. When the discharge stops this modified gas goes back to its original condition. If now the discharges through the gas follow each other so rapidly that the modified gas produced by one discharge has not time to return to its original condition before the next discharge passes, the successive discharges will pass through this modified gas. If, on the other hand, the gas has time to revert to its original condition before the next discharge passes, then the discharges pass through the unmodified gas; we regard this as being accomplished by means of successive decompositions and recombinations of its molecules, analogous to those which, on Grotthus' theory of electrolysis, occur when a current passes through an electrolyte.

We regard the arc discharge as corresponding to the first of the preceding cases where the discharge passes through the modified gas, the spark discharge corresponding to the second when the discharge goes through the gas in its unmodified condition.

From this point of view, the explanation of the results of the experiments on the electrolysis of steam are very simple. The modified gas produced by the passage of the discharge through the steam consists of a mixture of hydrogen and oxygen, these gases being in the same condition as when the arc discharge passes through hydrogen and oxygen respectively, when, as we have seen, the hydrogen behaves as if it had a negative charge, the oxygen as if it had a positive one. Thus, in the case of the arc in steam, the oxygen, since it behaves as if it had a positive charge, will go to the negative, while the hydrogen, behaving as if it had a negative charge, will go to the positive electrode. We saw that this separation of the hydrogen and oxygen took place.

The correspondence between the quantities of hydrogen and oxygen from the electrolysis of the steam and those liberated by the electrolysis of water shows that the charges on the atoms of the modified oxygen and hydrogen are the same in amount, but opposite in sign to those we ascribe to them in ordinary electrolytes.

In the case of the long sparks where the discharge goes through the steam, since the molecule of steam consists of two positively charged hydrogen atoms and one negatively charged oxygen one, when the molecule splits up in the electric field the hydrogen will go towards the negative, the oxygen towards the positive, electrode, as in ordinary electrolysis. We saw (p. 103) that for long sparks through steam the hydrogen appeared at the negative, the oxygen at the positive, electrode.

I have much pleasure in thanking Mr. E. Everett for the assistance he has given me in the course of the preceding investigation. ·

III. " On the Geometrical Construction of the Oxygen Absorption Lines Great A, Great B, and α of the Solar Spectrum." By George Higgs. Communicated by R. T. Glazebrook, F.R.S. Received February 20, 1893.

[Publication deferred.]

IV. " Upon the Existence of more than one Fungus in Madura Disease (Mycetoma)." By Rubert Boyce, M.B., M.R.C.S., Assistant Professor of Pathology, University College, London, and Nusserwangi Fakirgi Surveyor, M.D., M.R.C.P. Communicated by Professor Victor Horsley, F.R.S. Received February 21, 1893.

(From the Pathological Laboratory, University College, London.)

(Abstract.)

Nature of Mycetoma.—A very chronic, locally spreading inflammation of the foot, much less commonly of the hand ; characterised by the destruction of the tissues, great overgrowth of granulation tissue, and by the presence of very numerous *brown-white*, fish-roe-like particles, or more rarely of *black* particles.

Views held concerning Mycetoma.—In 1874, Carter held that the " fungus foot " was a veritable parasitic disease, due to the growth and extension, within the tissues, of an " indigenous mould." He came to the conclusion that it was one species, the *Chionyphe Carteri.* Lewis and Cunningham (1888) concluded that mycetoma was " essentially a degeneration of the fatty tissues, independent of the local presence or influence of any parasites whatever." Bassini (1888) met with a case in Italy, the only one, as yet, observed in Europe, and concluded that the parasite was allied to the higher Fungi, either the Aspergilli or Mucorini. Most recently, Dr. Kanthack brought forward evidence to show the identity or close affinity of the parasite with that of actinomycosis.

Our Views.—That the black particles represent a curious metamorphosis of a large, branching, septate fungus ; whilst the white particles consist largely of caseous material and of the remains of a lowly organised fungus, presenting in very many instances some of the characteristics of the fungus of actinomycosis. That both fungi are pathogenic. *The following observations* in support of these views are based upon an

examination of seven specimens of the black variety and of eighteen of the white, obtained from Bombay and from the various museums throughout the United Kingdom.

Black Variety.—Sections of the particles, free or *in situ* in the tissues, show that they are composed of tufts of a deep brown colour, and apart from a faint radiation or the presence of a slight venation or of holes, they give very little indication of a vegetable structure. By boiling the particles for from a few minutes to one hour in concentrated caustic potash, the brown colouring matter is very slightly removed, but this is completely discharged upon transferring them to distilled water; the fungus can then be readily studied. The animal tissues are however, destroyed by this process. If, however, the tissue containing the particles is embedded in collodion, washed for about one minute in " *eau de Javel*," and then stained, the colouring matter is removed from the fungus, and its relationship to the tissues around can be readily seen. The *fungus* appears the same in all the specimens of the black; the hyphæ radiate and branch, the segments vary very greatly in size; they may be spherical and reach a very great size, or long and slender; a pseudo-parenchyma may be formed in the centre of a tuft, or a palisade at the periphery. We have seen no organs of fructification. *Tissue reaction.*—The tufts are embedded in granulation tissue or necrosed material, and the presence of very large giant cells and other phagocytes is characteristic. The hyphæ may penetrate the vessels and run in their interior. The *metamorphosis* of the fungus appears to take place very early and affects equally the various hyphæ throughout the tissues. The nature and meaning of the change is very obscure. The dried particles burn with a luminous flame. Incinerated, there is a slight smell of burnt feathers, and in the ash, which is very little, there is a brown coloration, owing to the presence of iron; the presence of the latter may be confirmed in the unclarified and clarified specimens by the Prussian blue test. The iron is, however, limited to the periphery of the tufts, and appears wholly derived from the animal tissues. The particles give a red reaction with dilute nitric and hydrochloric acids: little impression is produced upon them by boiling in the various fat solvents; they give no special reactions with ferric chloride or cupric acetate (resin test).

White Variety.—Sections of the particles are characteristic. In the centre are usually numerous small reniform deeply-staining masses, surrounded by a deep radiate zone. In the central bodies a very fine reticulum may occasionally be made out; more usually, stronger evidence of the fungus is obtained by the presence of dwarfed club-like hyphæ, which form an irregular fringe to the reniform bodies. It is exceedingly difficult to ascertain what gives rise to the deep radiate zone; the leucocytes in it are compressed, yet the compressing

hyphæ remain for the most part unstained. The fungus undergoes very early degeneration. *Tissue reaction.*—The particles are surrounded by leucocytes and are either embedded in granulation tissue or lie free in the abscess cavities or sinuses.

In both varieties the spread of the particles and inflammation goes hand in hand, and a recurrence of the particles and of the inflammation has been observed by one of us in the scar left after amputation for the black variety of fungus foot.

Presents, March 9, 1893.

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Medal struck in commemoration of the Columbian Celebration.
    Sent by the Delegate-General of the Historico-American Exhibition, through Foreign Office.

Original and enlarged Drawings of a portion of the Solar Spectrum.
    Prof. A. S. Herschel, F.R.S.

March 16, 1893.

(In the Theatre of the London University.)

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

Professor Rudolf Virchow, who was elected a Foreign Member in 1884, signed the obligation in the Charter Book and was admitted into the Society.

The CROONIAN LECTURE was delivered as follows:—

" The Position of Pathology among Biological Studies." By Professor RUDOLF VIRCHOW, For. Mem. R.S.   Received March 3, 1893.

(Translation.)

It is now nearly ten years since this illustrious Society conferred on me the unexpected honour of electing me one of its Foreign Members.   Not only so, but last autumn it held me worthy of a further honour, in awarding me the Copley Medal,—a sign of the highest recognition of my work, the significance of which far exceeds the distinctions which the passing favour of political powers is accustomed to bestow.   Nevertheless, deeply as I appreciated this mark of its constant and increasing esteem, I was not in a position to offer my thanks personally to the Society. Numerous duties, official and private, the weight of which has increased with each year, kept me to continuous work at home, and even during the vacations the freedom of my movements has been for some time past restricted by international engagements, which yearly become more numerous and more pressing.

With great indulgence, which I fully know how to appreciate, the Council has allowed me to postpone the date of my appearance in your midst.   Hence, it is only to-day that you see me among you, and that I am able to tell you in person how very grateful I am to this Society, and how great an incentive to new efforts your recognition has been to me.

Who of us is not in need of friendly encouragement in the changing events of life?   True, happiness is not based on the appreciation of others, but on the consciousness of one's own honest labour.   How otherwise should we hold our ground in the midst of *the turmoil of the day?*   How should we preserve the hope of

progress and of final victory, in face of the attacks of opponents and the insults which are spared to nobody who comes before the public? He who during a long and busy life is exposed to public opinion, certainly learns to bear unjust criticism with equanimity, but this comes only through the confidence that his cause is just, and that some day it must triumph. Such is our hope in our wrestlings for progress in science and art. Such is our hope in our struggles for civil and religious liberty, and in this hope we gradually become hardened against malicious attacks. It is a kind of immunisation which, I acknowledge, has also great drawbacks, for this hardening against unjust attacks leads very easily to a similar indifference towards just attacks, and, owing to the tendency to contradiction rooted in the nature of human thought, it finally leads also to indifference to praise and recognition. We withdraw again and again into ourselves, discontented with the world and with ourselves also; but who can so completely retire within himself that the consciousness of the insufficiency of human thought, and that the criticisms of opponents are justified, cannot penetrate the crust of even the most hardened reserve? Happy is he who has courage enough to keep up or regain his relations with other men, and to take part in the common work! Thrice happy he who does not lack in this work the flattering commendation of esteemed colleagues!

Such were the thoughts which filled my mind, as, looking forward to the present occasion, I reviewed my own life and the history of science, or, to use another expression, the fortunes of our predecessors. How often have I found myself in a state of despondency, overcome by a feeling of extreme depression! And the history of science—what long periods of stagnation and what numerous interruptions has it not experienced owing to the victory of erroneous doctrines! What has saved me is the habit of work, which has not forsaken me even in the days of outward misfortune—that habit of scientific work which has always appeared to me as a recreation, even after wearying and useless efforts in political, social, and religious matters.

That which has saved science is identically the same; it only appears to be different, because the co-operation of many is necessary to secure its advance; hence, the exalting and consoling thought that one nation after another comes to the front, to take its share in the work. When the star of science becomes dim in one country it rises sooner or later, to yet brighter glory, in another, and thus nation after nation becomes the teacher of the world.

No science, more often than medicine, has gone through these waxings and wanings of brilliancy; for medicine alone of all the sciences has, for more than 2000 years, found ever new homes in the course of a progress which, though often disturbed, has never been wholly arrested.

I 2

It would lead us too far to illustrate this with examples drawn from the entire past. It is enough for my present purpose to take the outlines of modern medicine as the object of our consideration. Such a sketch, cursory as it must be, ought at the same time to throw some light on the intellectual relations of both nations, English and German, for these have taken a prominent part in establishing the principles of modern medicine.

The downfall of the old medicine, the so-called humoral pathology, was brought about in the beginning of the 16th century. We, in Germany, are inclined to attribute to ourselves a decisive *rôle* in this memorable struggle.

It was a man of our race, Andreas Vesalius (or of Wesel), who transformed anatomy into an exact science, and who thus, at one stroke, created for medicine a solid foundation, which it has retained ever since and which, let us hope, it will never again lose.

But the principal blow to the old medicine, was struck by his somewhat elder contemporary Paracelsus, that charlatan, yet gifted physician, who removed from among the beliefs of mankind the doctrine of the four humores, which, *quasi*-chemical in its construction, formed the basis of the old pathology. Strangely enough, he accomplished this with weapons borrowed from the armoury of the Arabs, the successors of the Greeks, and the chief representatives of the mediæval humoral pathology. From them, also, he borrowed alchemy, and, at the same time, the fantastic spiritualism of the East, which found a clear expression in his doctrine of the " Archœus," as the determining force in all living beings.

In this way, the new medicine, at its very birth, absorbed the germs of that ruinous antagonism, which, even up to the present century, has kept up the embittered strife of the Schools.

To Vesalius is due the exact spirit of inquiry which starts from the observation of actual conditions, and which, without further definition, we may call the anatomical.

Paracelsus, who pronounced the anatomy of the dead body to be useless, and sought for the basis of life as the highest goal of knowledge, demanded " contemplation " before all else; and, just as he himself arrived in this way at the metaphysical construction of the archæi, so he let loose among his followers a wild and absolutely fruitless mysticism.

Nevertheless there lay hidden in that " contemplation " of his a healthy kernel, which would not allow the intellectual activity which it had stirred up to sink to rest. It was the idea of *life*, which formed the ultimate problem for all future research. Strangely enough, this idea, which always existed in the popular mind, and which is *in an* unmistakable form present even amongst primitive nations, had *in scholastic medicine* been driven far into the background. Ever

since the time of Hippocrates, it had been the custom to use, instead of life, the obscure expression φύσις, *natura* ; but it is vain to seek for a more exact definition of the term. To Paracelsus nature was living, and the basis of his life was that very "archæus," a force differing from matter, and separable from it, or, as he himself expressed it, in the sense of the Arabs, a spirit, "spiritus." In the compound organism of man, the mikrokosmos, each part, according to him, had its own "archæus," but the whole was ruled by the " archæus maximus," the "spiritus rector." From this premiss has proceeded that long succession of vitalistic schools, which, in ever-changing forms, and with ever new nomenclature, introduced into the notions of physicians this idea of a fundamental principle of life.

If the sagacious George Ernest Stahl, whose services to the development of chemistry are now universally acknowledged, substituted the soul for the " spiritus rector," and so created a system of animism, the last vestiges of which have only within our own time disappeared from the school of Montpellier, so also in turn did the pure vitalists build up on the dogma of specific dynamic energies, maintained so stoutly by the physicists, that notion of the vital force, the half spiritualistic and half physical character of which has contributed so much, even in our day, to puzzle and mislead men's minds.

The doctrine of the vital force found its strongest support in the " Natur-philosophie," especially in that which, on German ground, soon obtained universal sovereignty.

This summary exposition of mine has greatly anticipated the historical progress of the evolution of medicine. It is now time to pay proper homage to the great investigator who made the more exact method the ruling one, and at the same time to award to this country, which brought him forth, its important share in determining the new direction of our science.

Nearly 100 years had passed since Vesalius and Paracelsus had begun their work when William Harvey published his 'Exercitatio anatomica de motu cordis et sanguinis in animalibus.' Here, for the first time, the anatomical examination of living parts was carried through, in an exemplary way, according to experimental methods. All the objections founded on the doctrine that anatomy concerned itself with dead parts only were thus at once set aside; living action became the object of immediate observation, and this was done on one of the most important organs, one absolutely necessary to life, the varying activity of which constantly calls for the attention of the practical physician. Not only so, but a new mode of observation— the experimental method—was thus brought into use for research ; a method by means of which a new branch of medical science, physiology, has been laboriously built up.

The influence of this one wonderful discovery of Harvey's on the ideas of men of his time, and of his successors, was memorable.

Among the men of his time the last support of Galenism disappeared with the proof of circulation; upon his successors the comprehension of the causation of local processes dawned for the first time. Very ancient and highly difficult problems, such as inflammation, could now be attacked; a goodly piece of life also became intelligible, since one of the vital organs themselves could now be subjected to experiment, and, to the astonishment of all, the action of this organ showed itself to be an absolutely mechanical one. The revulsion of thought was so complete that it has since become an almost insuperable difficulty to enter even in imagination into the ideas of the older physicians, to whom the circulation of the blood was unknown.

Nevertheless, in spite of such striking results, the craving of men for more complete understanding remained unsatisfied. The action of the living heart could be seen, but how did the heart live? What was this life, the action of which was so clearly visible? In the heart itself, the essence of life could not be recognised.

Harvey turned his attention to another object; he tried to observe the very beginnings of life in the incubated egg of the fowl and in the embryos of mammalian animals. He thereby soon arrived at the question of the significance of the egg in general, and enunciated the celebrated dictum, "Omne vivum ex ovo." Owing to the more extensive researches of modern investigators, this dictum, as is well known, proved too narrow for the whole animal kingdom, and no longer exact when applied to plant life. Its validity for the higher animals, on the other hand, cannot be questioned, and it has formed one of the firm standpoints from which researches on sexuality and on the propagation of life have proceeded. But Harvey, on account of the defective character of his optical instruments, was unable to see that which he was labouring to discover, namely, the process of organisation as such, just as in former times he had been unable to see the continuity of the capillary flow. This imperfection lasted for a long time afterwards; and thus it happened that even Albrecht von Haller and John Hunter considered the formation of the area vasculosa in the incubated egg of the fowl as the commencement of organisation, and indeed, as the type of organisation itself.

I will return to this point later; but I should like first to draw your attention to a man whose importance for the further development of the doctrine of life has always appeared to me to have been uncommonly great and highly significant, but who, nevertheless, has sunk into unmerited oblivion, not only among posterity in general, but also, I think I may be allowed to say, even among his countrymen. I mean Francis Glisson, who was a *contemporary of Harvey*, and whose works appeared almost simulta-

neously with those of his more celebrated colleague ; but the brilliancy of Harvey's discoveries was so great that the light which shone from Glisson's work-table almost disappeared. I rejoice that on so auspicious an occasion I may recall the memory of the modest investigator, and may offer him the tribute of gratitude which science ought long since to have awarded to him.

When, thirty-five years ago, I published my little essay on "Irritation and Irritability" ('Archiv für Pathologische Anatomie und Physiologie,' 1858, vol. xiv, p. 1), I did not know much more about Glisson than what every student of medicine learns, namely, that there is in the liver a "capsula communis Glissonii," and, what was even less known, that this anatomist had written a small work on 'Rachitis,' which, indeed, was the first of its kind. In my own paper on this disease (*ibid.*, 1853, vol. v, p. 410) I had tried to demonstrate the circumspection and accuracy which are noticeable in this book, and which make it a typical model for all collective investigations ; but even at that time I overlooked the fact that this was only the smallest merit of this wonderful man. It was only in the further course of my studies on the history of the doctrine of irritation and irritability that I made the discovery, astonishing to me, that the idea of irritability did not, as is generally thought, originate with Haller, but that the father of modern physiology, and the Leyden School in which he had been brought up, had borrowed this idea from Glisson. I then stumbled on a series of almost forgotten publications of this original scholar, especially his 'Tractatus de natura substantiæ energeticæ, seu de vita naturæ ejusque tribus primis facultatibus, perceptiva, appetitiva et motiva,' which appeared in London in 1672, wherein the ideas were further worked out, the outlines of which had already been brought forward in his 'Anatomia hepatis,' published in 1654. In this work (p. 400) the newly-coined word "irritabilitas" appears, so far as I can find out, for the first time in literature. It may be noticed, by the way, that the expression "irritatio" is much older. I find it already in Celsus, but with an exclusively pathological signification. It appears, also, occasionally in later writers, and to this day it has not, speaking accurately, lost this original signification. It is otherwise with Glisson ; to him, irritability is a physiological property, and irritation merely a process of life dependent on the natural faculties of living matter.

Thus he was led, through a process of "contemplation," to maintain the existence of the "biarchia," the "principium vitæ," or the "biusia," the "vita substantialis vel vitæ substantia." And in order to allow of no misunderstanding as to the source of his "contemplation," he adds distinctly that this is the "archæus" of Van Helmont —the "vis plastica" of plants and animals.

In the further ▬▬▬▬▬▬▬ philosophical discussions, he is ▬▬▬▬▬▬

nevertheless, into the same by-path, into which, even in the most recent times, so many learned men and even excellent observers have been misled. This is the path of unlimited generalisation. The human mind is only too prone to render intelligible what is unintelligible in particular phenomena, by generalising them. Just as even in recent times an attempt has been made to render consciousness intelligible by representing it as merely a general property of matter, so Glisson thought he might attribute to the active principle ("principium energeticum") which according to him is contained in all matter, the three faculties of living matter which he considered as fundamental, namely, the facultas perceptiva, appetitiva et motiva. All matter was sensitive, was thus stimulated to develop impulses, and moved itself as a consequence of these impulses.

It is not necessary for the purpose of our present enquiry to carry these quotations further, since they are quite, in the Paracelsian sense, contemplative in their nature; and especially as, in their generalisation, they do not appear to be important for the history of advancing knowledge.

That which is full of significance for us is concerned with actual life only, in the narrower sense of analytic science. It was not the "principium energeticum" set up by Glisson, which stimulated his successors again to take up the thread of his observations, but rather this process of irritation described by him, together with the fundamental faculties of living matter on which it depended. In this way he has really led up to a more exact study of the actions of life and the properties of living matter.

Unfortunately, there intervened a mistaken conception, which led his followers again into a series of most serious errors. Glisson, following on this point also the example of Van Helmont, was convinced that nerves contracted when irritated. He added to this the idea that, through the contraction of the nerves, or even of the brain, the fluid contained in them was propelled towards the periphery.

This notion, shared by Willis and many other physicians of that time, furnishes the reason why irritability was identified with contractility. Even the great master Hermann Boerhaave, and after him his pupil Gaubius, the first special writer on general pathology, considered sensation and motion as common properties of, at all events, all the solid parts of the body. The former thought it proved that hardly a single particle of the body existed which was not sensitive and did not move; and thus it becomes comprehensible how Haller himself carried this idea, that irritability had the same significance as contractility, from his school days in Leyden to his professorship in Göttingen. It was in this sense that he understood the irritability of the muscles, and in the same sense he *denied this property to* the nerves.

This dispute about the irritability of muscles has continued far into the present century; its long duration becomes intelligible only when we bear in mind that, without the most exact knowledge of its historical development, even the very statement of the question is liable to be misunderstood.

As a matter of fact, so far as we know, the nerves are not contractile, like the muscles; on the other hand, the muscles are not only contractile, but are also irritable. Irritability and contractility are not identical, even when they occur in the same part. The nerve current, on the other hand, cannot be compared with the blood stream: it does not consist in the movement of a fluid, but is of electrical nature, and hence there is no need for its production of a contraction of the nerve-tubes.

It was also an erroneous conclusion that every irritated part contracted. Instead of contraction, secretion, or, under certain circumstances, a more vigorous nutrition, may occur as the final result of irritation. Hence we use a more comprehensive term in order to express this final result, and call all forms of it "actions." While Glisson defined all " actio propria sic dicta " as " motus activus," we distinguish different kinds according to the nature of the effects, or, otherwise expressed, according to the direction of the activity (nutrition, formation, and function); but we agree with the above thinker in the opinion that no vital energy is ever set free without stimulus: and that, consequently, every action is of an irritative nature. In this irritation, according to my idea, consists the " principium dividendi," according to which we must distinguish between active and passive processes of life, and in this way we gain also a basis for the fundamental division of pathological elementary processes. How much work has been necessary in order to render this conception possible! And how great, even now, is the number of our colleagues who have not fully accepted it! The reason for this difficulty is two-fold.

Most of the vital actions of life, whenever they manifest themselves by visible events, are of a compound nature. As a rule, parts very various, at times wholly unlike, each with its specific energy, combine to produce them. Not unfrequently it thereby happens that in the visible sum of final effects one part behaves in an active, the other in a passive, manner. It is only the most minute analysis of the phenomenon, tracing it right back to the elementary parts, which allows the total result to be resolved into its components; such an analysis cannot, as a rule, be expressed in current language, except at great length. No language in the world is rich enough to possess special expressions for each such combination. Only too often we help ourselves out of the difficulty by regarding the compound phenomenon as a *simple one*, and by expressing its character accord-

ing to some chief trait, which stands out in a commanding manner from the general picture. This is the practical difficulty.

With it, however, a theoretical difficulty is very often combined. The human mind, owing to a natural impulse, seeks in the phenomena indications of their determining cause. The more complex the phenomenon, the more busy is the imagination, in order to convert it into a simple one, and to find for it a unitary cause. So has it been in respect to life, so also in respect to disease. The course of thought followed by Glisson is opposed to such an explanation. He had no scruple in dividing the unit of life into a large number of individual lives. Although the knowledge we now possess of the arrangements of the body was absolutely foreign to him, yet he arrived quite logically at the vita propria, the proper elementary life, of the several parts. To be sure, this expression, as far as I can see, is not to be found in his works, and occurs first in those of Gaubius; but Glisson says distinctly:* "Quod vivit per se vivit vitam a nulla creatura præter se ipsum dependentem. Hoc enim verba vivere per se sonant."

The unitary efforts of the following period relentlessly passed over the tendency of which I have just spoken. Some returned to the old Mosaic dictum, "the life of the flesh is in the blood"; others gave the nervous system, and the brain especially, the first place in their consideration. Thus once more began the old struggle, which for thousands of years had divided the schools of medicine into humoral and solidar pathology. Even when we ourselves entered on scientific work, hæmato-pathologists stood in hostile attitude to neuro-pathologists.

In England, humoral pathology found a strong support in the great and legitimate authority of John Hunter. Although this distinguished practitioner never shared the one-sidedness of the later pathologists, but rather attributed to the solid parts the living principle the existence of which he assumed, yet, in his investigations, the blood took precedence over all other parts as the chief vehicle of life.

One must, however, recall to mind that Hunter laid special stress on the fact that life and organisation are not bound to each other, since animal substances which are not organised can possess life. He started, as has already been noticed, from the erroneous conception that eggs are not organised, and that it was not till after incubation that the first act of organisation, namely, the formation of vessels, took place. He considered his "diffuse matter" ("materia vitæ diffusa") as the actual carrier of life; and this was to be met with not only in the solid parts, but in the blood also. This matter, according to him, existed in the brain in a remarkable degree of concentration, but

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* *Glisson,* ' Anatomia hepatis,' "Ad lectorem," N. 17.

its presence was quite independent of all nervous structures, as is shown by the example of the lower animals which possess no nerves. In the posthumous writings of Hunter, which Owen has collected, the very striking expression " simple life " is met with, a state most clearly to be recognised in plants and the lowest animals. This simple life was in Hunter's view the ultimate source of all living activities, pathological as well as physiological.

Hunter was out and out a vitalist, but his materialistic vitalism, so to speak, differed *toto cœlo* from the dynamic vitalism of the German schools. If living matter existed independently of all organisation, such living matter was beyond the scope of anatomical investigation; but, on the other hand, if it were present in non-organised parts, such as an egg, it was in itself the ultimate source of the organisation which subsequently makes its appearance in these parts. It must, therefore, to adopt a later mode of expression, be of a plastic nature. Here Hunter's notion fell in with that of the plastic lymph, as developed by Hewson; and it was only logical that Schultzenstein at last applied it to the blood, and designated as " plasma " the material of life present in the blood. In this way the formative and nutritive matter necessary to physiological life, as well as the plastic exudations occurring in diseased conditions, could be attributed to the same material—a highly satisfactory result in appearance, and one providing a most convenient basis for interpretations. The exponents of this notion had no scruples in going one step further, and in providing this material of life with a technical name. They called it " fibrin." Evidently this did not quite correspond with Hunter's ideas, for we know of no such matter, either in the egg or in the plants or the lower animals, as that to which he attributed simple life; but the exigencies of pathology overcame all such scruples, and the plastic exudations were received as undoubted evidence that fibrin possessed the power of becoming organised. They formed, in the *crasis* doctrine of the Vienna School, the bright spot in the history of this newest kind of hæmato-pathology.

Wherever fibrin failed, blastemata were brought to the fore. Ever since Schwann had given the name of cyto-blastema to the organising material of the egg, the way had been open for assuming, in other places, the existence of material with this ambiguous name.

But of course through these steps the one simple matter of life predicated by Hunter was replaced by many " matters of life," and thus the entire advantage gained by the exposition of a unitary theory of life was at once lost.

Even when, finally, the cell-contents were designated as protoplasm, and thus the one requisite of Hunter, namely, that the material of life must also be contained in the individual parts, appeared to be fulfilled, yet no *single specific* material was thereby arrived at. No

one dreamed of regarding protoplasm as fibrin, and least of all did any one consider it a simple chemical body.

By the conception of the blastema, however, there had been re-awakened a thought which had occupied men's minds from the earliest times. If a plastic matter capable of being organised really existed in the body, then the organisation of the same must present the first reliable example of epigenesis. The problem of the "generatio æquivoca," so long fought over, now appeared to be solved. What Harvey had taught concerning the continuous descent from the egg became temporarily obliterated when the theory of descent through exudation made its appearance. Several generations of young medical men have been educated in this belief. I myself remember my "epigenetic" youth, with no little regret, and I have had hard work to force my way through to the recognition of the sober truth.

Meanwhile, the attention of other bodies of inquirers had been directed to the tissues of the body. Among these, in view of their importance, the nervous tissues, and especially the mass of nervous tissues in the brain and spinal cord, rank highest.

Hunter also had acknowledged the importance of the brain, and hence called it the "materia vitæ coacervata." It was easily seen that it contained no fibrin, but experimental research showed also that neither the brain nor the spinal cord was of the same value through-out all its parts. The more accurate the experiments, the smaller became the region which, in the strictest sense, is the vital part, until Flourens limited it to one single spot, the knot of life (" nœud vital "). Was the unity of life found in this way ? By no means. The brain is no more and no less vital than the heart; for life is present in the egg long before the brain and heart are formed, and all plants, together with an immense number of animals, possess neither the one nor the other. In the highly compound organism of man, the brain and spinal cord have a certain determining action on other parts necessary to life. Their disturbance may immediately be followed by the disturbance of other vital organs, and sudden death may ensue.

But the collective death of a compound animal no more implies the immediate local death of all its special parts than the local death of some of the latter is incompatible with the continued collective life of the animal. As has been well said, at the death of a compound organism there is a "primum moriens," one part which first ceases to live ; then follow, sometimes at long intervals, the other organs, one after the other, up to the "ultimum moriens." Hours and days may pass between the total death of the individual and the local death of the parts. The fewer nerves a part contains, the more slowly usually does it die ; I therefore consider the process of dying in the compound *organism as the best illustration* of the individual life of the several

constituent parts, which is in its turn the first axiom necessary for the study and the understanding of life.

A long time, however, elapsed before it was possible to return to this starting point, and to obtain a considerable number of supporters for the doctrine of the " vita propria." The attention of many observers was drawn to a totally different side of the question. In the last decade of the past century, about the same time that John Hunter, starting from careful anatomical investigations and exact observations of surgical practice, worked out his idea of the material of life, a new system of medicine was founded in Scotland, the so-called Brownian system, which was based on quite different pre-misses. Brown also was a vitalist; he, too, constructed, not merely a pathological and therapeutic system of vitalism, but a physio-logical one, though this doctrine was dynamic in its character. There is but little to be noticed therein of the material anatomical foundation of exact medicine. It is principally concerned with contemplations of the forces of the living organism. One can under-stand to some extent how this happened, if the history of the deve-lopment of this extraordinary personality is kept in view ; I cannot go into this here, but anyhow the remarkable fact remains that the two contemporaries, Brown and Hunter, worked near each other without any evidence in their writings that they were acquainted with one another. Brown struck out his own line, and stuck to it, without troubling himself about the rest of the medical world. And yet even his first work, ' Elementa Medicinæ,' had the effect of an earth-quake ; the whole European continent was shaken by it, while the physicians of the recently opened New World bent under the yoke of revolutionary ideas ; and in a few years the aspect of the whole field of medicine was entirely changed. True, the triumph was but short ; the Brownian system disappeared as it had come, a meteor in the starry heaven of science. There would be no reason to go into it more fully, had not the impulse which it gave stimulated other men, by whom it was permanently applied to the true service of science. This impulse was founded on the fact that irritability, or, as Brown called it, " incitability," was thus reinstated as the starting point of the theory ; but, along with this, the stimuli which set living substances in action, the " potestates' incitantes," were brought to the fore. In so far as stimuli produce a state of irritation ("incitatio"), or, as Brown called it later, excitement, they came to be viewed not only as the cause of health and disease, but even of life itself ; for excite-ment, so he said, is the true cause of life. But, as excitement stands in a certain relation to the strength of the stimulus, a state of good health was only possible with a normal degree of stimulus, whilst an excess or a lack of stimulus brought diseased conditions in its wake. Of *course excitement is* dependent also on irritability, with a certain

quantity of which, in the form of energy, every living being is
endowed at the beginning of its life.

The division of diseases, according to the amount of vital force
visible in them, into sthenic and asthenic, has never since been
abandoned, though acknowledged perhaps in a less precise manner;
it has sometimes been brought more prominently forward, and some-
times thrown into the background. In Germany, Schönlein was the
one of all others who took this doctrine as the foundation for his
opinion on special cases of disease, and of his choice of treatment.

But the application of the Brownian principles to physiology has
been of far greater importance. If life itself were dependent on
external stimuli, the notion of the spontaneity of vital actions,
a notion still in force, must lose all significance. Certain stimuli
would in that case prove to be necessary conditions of vital
activity, without which life could at best be carried on in a latent
form only. Certainly even for this latent life the question remained
open: How does it come to pass, and in what does it practically
consist? Brown avoided this ticklish question, not without great
skill, by drawing the whole attention to active life and to the stimuli
which call forth action. To speak freely, science has since then
deflected little, if at all, from this guiding notion. Even now,
we cannot say what latent life is. We simply know that through
external stimuli it may be converted into active life, and hence irrit-
ability is considered by us as the surest sign of life, not of course of
the general life of all matter in the sense of Glisson, but of the real and
individual life of special living organisms. Brown remarked, with
reason, that through irritability the living substance is differentiated
from the same substance in its dead condition, or from any other
lifeless matter. Nevertheless, neither irritability nor incitability,
neither irritation nor incitation, explains the essence of the living
substance, and therefore neither explains the essence of life.

In Germany especially, the physiologists took up this question.
Among the first was Alexander von Humboldt, who in his various
writings, especially in his celebrated treatise on the irritated muscle
and nerve fibre, entered into the question. In the end he held fast
to the assumption of a vital force. The majority of pathologists and
physicians followed in his footsteps, and long and fierce controversies
were necessary before, nearly half a century later, the belief in a vital
force was destroyed. When du Bois-Reymond had demonstrated the
electrical current in muscle and nerve in all its characters, and, at
the end of his work, had also disclosed the inadmissibility of vital
force, then the venerable Humboldt formally and expressly renounced
the dream of his youth, with the masterly submission of the true
naturalist to the recognised natural law.

The hypothesis of a particular force of life had, however, in regard

to Brown's theory neither a positive nor a negative value.   Johannes Müller rescued for general physiology, in which it has ever since kept its place, that which was valuable in Brown's system, the doctrine of the integrating life stimuli.   The occasional stimuli which produce disease have found their place in etiology; their significance has become more and more sharply defined, the more accurately we have learnt to distinguish between the causes and the essences of disease, a distinction which became more difficult as the "causæ vivæ" of diseases became known in ever-increasing numbers.   And now a new task has arisen, namely, to draw into our sphere of observation the life of the causative agents themselves.

The way in which pathology has tried to approach the desired goal, to fathom the living substance in its diseased conditions, has led us a great step forward.   Pathological anatomy, especially, has opened this road.   The more numerous its observations, and the more it penetrated into the details of the lesions, the smaller became the field of so-called general diseases.   The first steps of mediæval anatomists had the effect of drawing the attention to local diseases.   In the first and longest period, which we may define as that of Regionism, the pathological anatomists sought the cause of disease in one of the larger regions or cavities of the body—in the head, chest, or abdomen.   In the second period, ushered in by the immortal work of Morgagni, shortly before the time of which I last spoke—the time of Brown and Hunter—they endeavoured to find in a certain region the actual organ which might be considered as the seat of disease.   On this foundation arose the Parisian school of Organicism, which, until late in this century, held a dominant position in pathology.   In this school, already, they recognised that not the organ, nor even a portion of it, could be the ultimate object of research.   Xavier Bichat divided the organs into tissues, and showed that in the same organ sometimes one and sometimes another tissue might be the seat of disease.

From that time forward the eye of the pathological anatomist was directed chiefly to the changes in the tissues, but it soon became apparent that even the tissues are not simple substances.   Since the third decade of this century, the microscope has disclosed the existence of cells, first in plants, and very soon afterwards in animals. Only living beings contain cells, and vegetable and animal cells show so much similarity of structure that one can demonstrate in them the actual product of organisation.   This conviction has become general, since through our embryologists, especially through Schwann, proof has been afforded that the construction of embryonic tissues is derived from cells both in the highest animals and in man himself.

In the fourth decade of this century the science of pathological anatomy had already begun to be directed towards cells.   These re

searches very soon encountered great difficulties. Many tissues, even in their developed state, appeared to contain neither cells nor their equivalents; nevertheless, I have been able to demonstrate their existence in those tissues in which their presence appeared to be most doubtful, viz., in bone and connective tissues. At the present time we are so far advanced as to be able to say that every living tissue contains cellular elements. We go a step further even, for we require that no tissue should be called living in which the constant occurrence of cells cannot be shown.

A still greater difficulty then appeared, namely, to discover in what way new cells originated. The answer to this question had been very heavily prejudiced by the so-called cell-theory of Schwann. Inasmuch as this very trustworthy investigator asserted that new cells originated from unformed matter, from " cyto-blastema," there was opened up a wide road to the old doctrine of the " generatio æquivoca," which afforded all partisans of plastic materials an easy way of reviving their dogma. The discovery of cells of connective and allied tissues gave me the first possibility of finding a cellular matrix for many new growths. One observation followed another, and I was soon in a position to give utterance to the dictum, " Omnis cellula a cellula."

And so at last the great gap was closed which Harvey's ovistic theory had left in the history of new growth, or, to speak more generally, in the history of animal organisation. The begetting of a new cell from a previous cell supplements the reproduction of one individual from another, of the child from the mother. The law of the continuity of animal development is therefore identical with the law of heredity, and this I now was able to apply to the whole field of pathological new formation. I blocked for ever the last loophole of the opponents, the doctrine of specific pathological cells, by showng that even diseased life produced no cells for which types and ancestors were not forthcoming in normal life.

These are the fundamental principles of cellular pathology. In proportion as they have become more certain, and more generally recognised, they have in turn become the basis of physiological thought. The cell is not only the seat and vehicle of disease, but also the seat and carrier of individual life; in it resides the " vita propria." It possesses the property of irritability, and the changes in its substance, provided these do not destroy life, produce local disease.

Disease pre-supposes life; should the cell die, its disease also comes to an end. Certainly, as a consequence, the neighbouring and even far-distant cells may become diseased, but as regards the cell itself the susceptibility to disease is extinguished with life.

Since the cellular constitution of plants and animals has been proved, and since cells have become recognised as the essentially

living elements, the new science of biology has sprung up. It has not brought us the solution of the ultimate riddle of life, but it has provided concrete, material, anatomical objects for investigation, the structures and active and passive properties of which we can analyse. It has put an end to the wild confusion of fantastic and arbitrary notions such as I have just mentioned ; it has placed in a strong light the immeasurable importance of anatomy, even in the most delicate conditions of the body, and lastly, it has made us aware of the close similarity of life in the highest and lowest organisms, and has thus afforded us invaluable means for comparative investigation.

Pathology has also its place, and one certainly not without honour, in this science of biology, for to pathology we are indebted for the knowledge that the opposition between healthy and diseased life is not to be sought in a fundamental difference of the two lives, not in an alteration of the essence, but only in an alteration of the conditions.

Pathology has been released from the anomalous and isolated position which it had occupied for thousands of years. By applying its revelations not only to diseases of man, but also to those of animals, even the smallest and lowest, and to those of plants, it in the best manner helps to strengthen biological knowledge, and to narrow still more that region of the unknown which still surrounds the intimate structure of living matter. It is no longer merely applied physiology ; it has become physiology itself.

Nothing has more contributed thereto than the constant scientific union which has endured for more than three hundred years between English and German investigators, and to which to-day we add yet another link. May this union never be broken !

March 23, 1893.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and
Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks
ordered for them.

The following Papers were read:—

I. " On the Composition of Water by Volume." By ALEXANDER
   SCOTT, M.A., D.Sc., Jacksonian Demonstrator in the Uni-
   versity of Cambridge. Communicated by LORD RAYLEIGH,
   Sec. R.S. Received March 4, 1893.

(Abstract.)

In a preliminary note presented to the Society in June, 1887, the
results of twenty-one experiments on the composition of water by
volume were given in detail. The ratio deduced from these experi-
ments was less than two volumes to one of oxygen. This result was
unexpected, because of the greater compressibility of oxygen than of
hydrogen, but as every one of the experiments pointed to this result,
the evidence for it seemed conclusive. Pursuing the investigation
with improved apparatus, especially as regarded making a complete
analysis of the residual gas, a serious source of error was discovered
in the use of any combustible lubricant for the taps employed. On
substituting syrupy phosphoric acid for the vaseline previously em-
ployed, the oxides of carbon disappeared as ordinary impurities. In
the later experiments two forms of apparatus were employed, the
chief difference being that in the earlier form the measuring vessel
was not of fixed volume, so that both volume and pressure of the
gas had to be measured; in the later form the gas was measured at
constant volume by varying the pressure, which alone, therefore, re-
quired measurement in each experiment.

As in the apparatus formerly used, the entire apparatus could be
completely exhausted of air before beginning an experiment by using
the mercury reservoir (M_1) and the measuring vessel as a Töppler's
pump. The gases were measured in A and B, and after measurement
were mixed in the jar H, whence they were drawn into the explosion
tube J, and then exploded in fractions till all was used up. The residue
was now passed back into H, and then into B, and there measured,
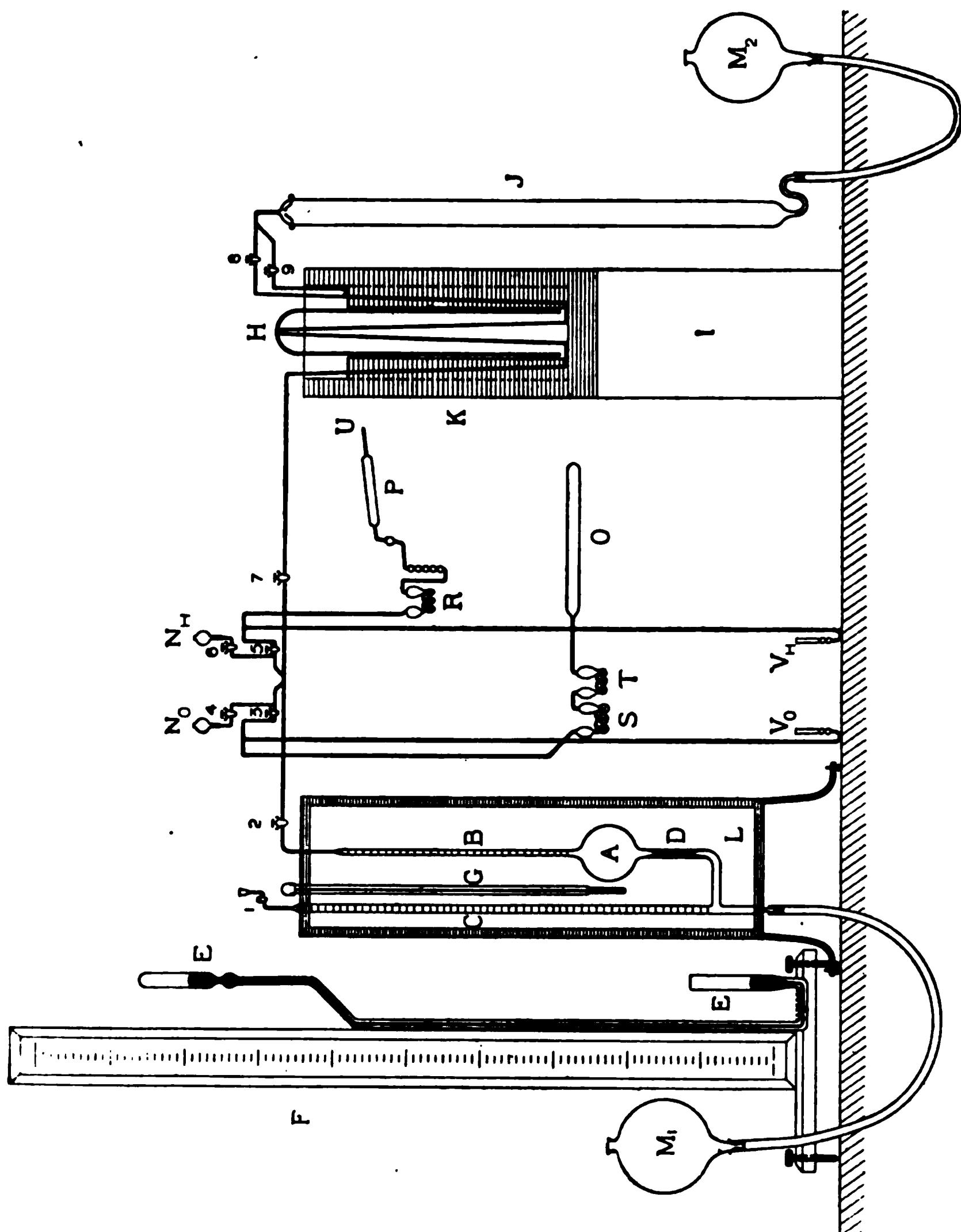
retransferred to J, and expelled through the tap 9 into an absorption tube, and analysed with potassium hydrate and pyrogallol for carbon dioxide and oxygen, and for gases not absorbed by these reagents. These gases were most probably nitrogen, hydrogen, and carbon monoxide (from the absorbents). The mixture thus obtained gave at any rate a *maximum* value for any impurity in the gases employed; when it fell below 1/110,000 the gases were considered pure.

The hydrogen required was made from

    1. Electrolysis of dilute sulphuric acid.
    2.   „   hydrochloric acid.
    3. Action of steam on sodium.
    4. Palladium hydride.

The oxygen was obtained from

    1. Re-crystallised potassium chlorate.
    2. Mercuric oxide.
    3. Silver oxide.

All the results in which potassium chlorate was used as the source of the oxygen gave low results, doubtless due to traces of chlorine accompanying the oxygen even after passing through potassium hydrate. The best oxygen was obtained from silver oxide, and the best hydrogen from palladium hydride. The palladium used combined with enough hydrogen to perform twelve experiments in succession. The results of the last series are given in the following table. The oxygen required was obtained from silver oxide.

    Column A contains number of experiment.
       „   B contains date of experiment.
       „   C contains measured volume of hydrogen in grams of mercury.
       „   D contains measured volume of oxygen in grams of mercury.
       „   E contains excess of hydrogen in grams of mercury.
       „   F contains excess of oxygen in grams of mercury.
       „   G contains impurity in grams of mercury.
       „   H contains number of volumes of hydrogen which unite with one of oxygen.

A.	B.	C.	D.	E.	F.	G.	H.
XXV	Ap. 1	6863·8	3443·8	..	15·4	0·3	2·0020
XXVI	„ 1	6870·0	3432·9	..	2·1	0	2·0024
XXVII	„ 2	6870·1	3439·7	..	9·2	0	2·0026
XXVIII	„ 4	6848·7	3422·1	..	2·9	0	2·0030
XXIX	„ 4	6792·5	3386·6	13·5	..	0	2·0022
XXX	„ 5	6809·2	3399·5	1·5	..	0	2·0025
XXXI	„ 6	6793·9	3399·6	..	7·7	0	2·0029
XXXII	„ 6	6789·6	3389·5	2·9	..	0	2·0023
XXXIII	„ 7	6808·5	3396·4	6·0	..	0	2·0028
XXXIV	„ 8	6793·1	3395·8	..	2·1	0	2·0017
XXXV	„ 8	6786·5	3395·0	..	5·4	0	2·0022
XXXVI	„ 9	6814·8	3411·9	..	9·3	0	2·0028

Mean = 2·00245 ± 0·00007.

The mean of all the experiments in the variable volume apparatus, and in which potassium chlorate was the chief source of oxygen, is

2·000903 ± 0·00004 impurity equally distributed in both gases,
1·99925   ± 0·00005   „   assumed to be all in hydrogen,

in 5 series of 19 experiments in all.

The mean of all the experiments in the constant volume apparatus, in which silver oxide was used as the source of the oxygen, and sodium and steam either directly, or after absorption in palladium for the hydrogen, is

2·002435 ± 0·00006 impurity in both gases equally,
or   2·002431 ± 0·00006   „   hydrogen alone.

This is the mean of 53 experiments in 5 series.
If 6 experiments be rejected we get the value

$$2·002466 ± 0·000003$$

as the result of 47 experiments in 5 series, and any impurity makes no difference, whether it be assumed all in the hydrogen or equally distributed in both gases. The most probable value, however, is 2·00245.

This value, combined with the value 15·882 for the ratio of the densities found by Lord Rayleigh, gives for the atomic weight of oxygen

15·862

Dittmar and Henderson's value is.... 15·866
Cooke and Richards'   „   .... 15·869

Leduc[*] found for the ratio of the volumes, by taking the density of electrolytic gas from strong potassium hydrate solution and his own values for the densities of hydrogen and oxygen,

2·0037,

aud for the ratio of densities   15·905,

giving for the atomic weight of oxygen,

15·876.

Morley's experiments[†] are objected to on the ground that his apparatus is too complicated, his measuring vessel far too wide for accurate measurement and to its being used also as the explosion tube, the transferring of his gases from one mercury trough to another, and

* 'Comptes Rendus,' vol. 115, p. 313.
† 'Amer. Journ. Science,' vol. 41, Ser. 3, pp. 220, 276.

his giving no means of saturating the gases with aqueous vapour. He further measures his pressures to 1/200th of a millimetre. His ratio for the volumes is given as

$$2\cdot00023,$$

or only 1/10th of the difference from 2·0 exactly of that found by the above-described experiments.

II. " On the Densities of the Principal Gases." By LORD RAYLEIGH, Sec. R.S. Received March 4, 1893.

In former communications* I have described the arrangements by which I determined the ratio of densities of oxygen and hydrogen (15·882). For the purpose of that work it was not necessary to know with precision the actual volume of gas weighed, nor even the pressure at which the containing vessel was filled. But I was desirous before leaving the subject of ascertaining not merely the relative, but also the absolute, densities of the more important gases, that is, of comparing their weights with that of an equal volume of water. To effect this it was necessary to weigh the globe used to contain the gases when charged with water, an operation not quite so simple as at first sight it appears. And, further, in the corresponding work upon the gases, a precise absolute specification is required of the temperature and pressure at which a filling takes place. To render the former weighings available for this purpose, it would be necessary to determine the errors of the barometers then employed. There would, perhaps, be no great difficulty in doing this, but I was of opinion that it would be an improvement to use a manometer in direct connexion with the globe, without the intervention of the atmosphere. In the latter manner of working, there is a doubt as to the time required for full establishment of equilibrium of pressure, especially when the passages through the taps are partially obstructed by grease. When the directly con-nected manometer is employed, there is no temptation to hurry from fear of the entrance of air by diffusion, and, moreover (Note A), the time actually required for the establishment of equilibrium is greatly diminished. With respect to temperature, also, it was thought better to avoid all further questions by surrounding the globe with ice, as in Regnault's original determinations. It is true that this procedure involves a subsequent cleaning and wiping of the globe, by which the errors of weighing are considerably augmented; but, as it was not proposed to experiment further with hydrogen, the objection was of less force. In the case of the heavier

* 'Roy. Soc. Proc.,' February, 1888; February, 1892.

gases, unsystematic errors of weighing are less to be feared than doubts as to the actual temperature.

In order to secure the unsystematic character of these errors, it is necessary to wash and wipe the working globe after an exhaustion in the same manner as after a filling. The dummy globe (of equal external volume, as required in Regnault's method of weighing gases) need not be wiped merely to secure symmetry, but it was thought desirable to do so before each weighing. In this way there would be no tendency to a progressive change. In wiping the globes the utmost care is required to avoid removing any loosely attached grease in the neighbourhood of the tap. The results to be given later will show that, whether the working globe be full or empty, the relative weights of the two globes can usually be recovered to an accuracy of about 0·3 milligramme. As in the former papers, the results were usually calculated by comparison of each "full" weight with the mean of the immediately preceding and following empty weights. The balance and the arrangements for weighing remained as already described.

The Manometer.

The arrangements adopted for the measurement of pressure must be described in some detail, as they offer several points of novelty. The apparatus actually used would, indeed, be more accurately spoken of as a manometric gauge, but it would be easy so to modify it as to fit it for measurements extending over a small range.

The object in view was to avoid certain defects to which ordinary barometers are liable, when applied to absolute measurements. Of these three especially may be formulated:—

a. It is difficult to be sure that the vacuum at the top of the mercury is suitable for the purpose.

b. No measurements of a length can be regarded as satisfactory in which different methods of reading are used for the two extremities.

c. There is necessarily some uncertainty due to irregular refraction by the walls of the tube. The apparent level of the mercury may deviate from the real position.

d. To the above may be added that the accurate observation of the barometer, as used by Regnault and most of his successors, requires the use of a cathetometer, an expensive and not always satisfactory instrument.

The guiding idea of the present apparatus is the actual application of a measuring rod to the upper and lower mercury surfaces, arranged so as to be vertically superposed. The rod AA, fig. 1, is of iron (7 mm. in diameter), pointed below B. At the upper end, C, it

Fig. 1.
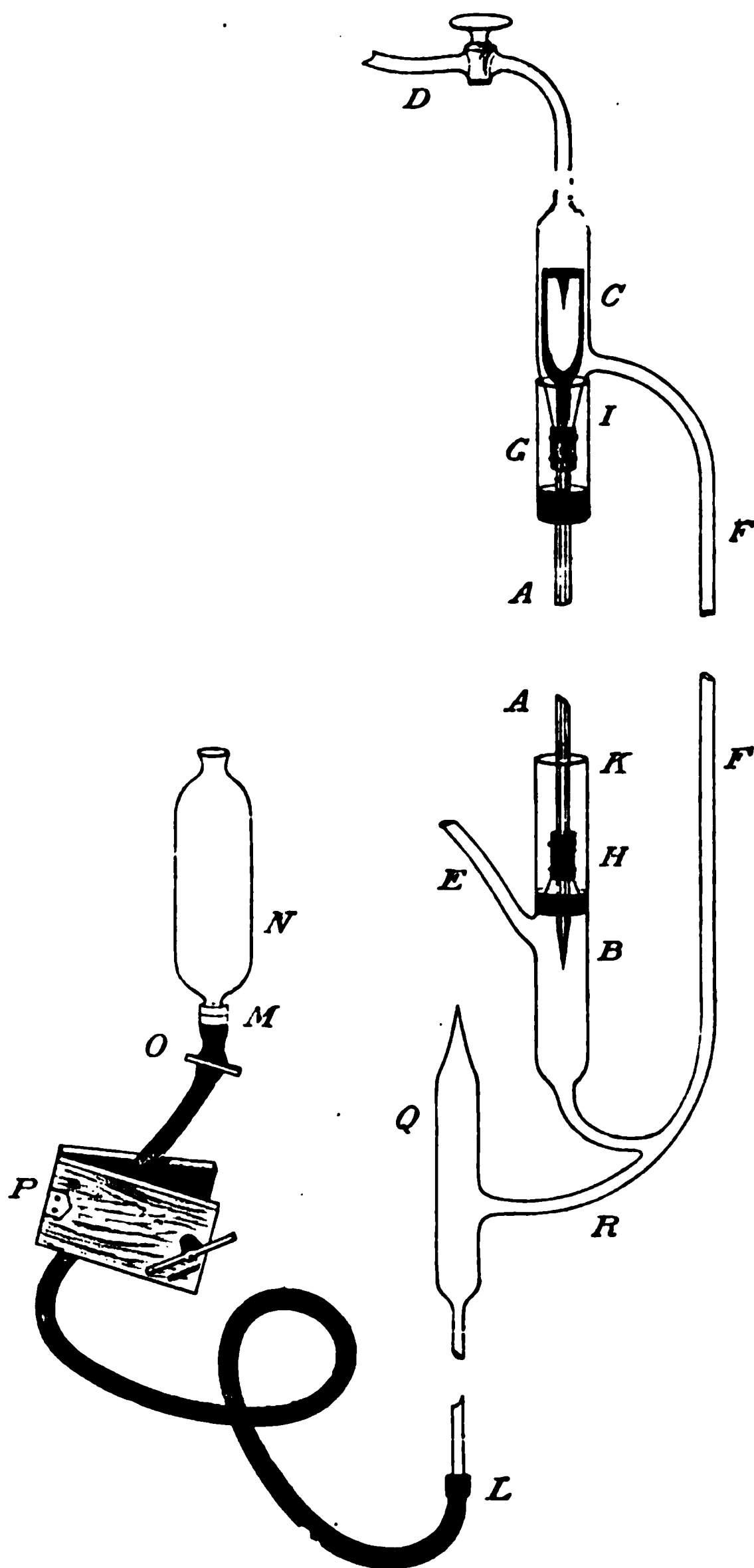
divides at the level of the mercury into a sort of fork, and terminates *in a point* similar to that at B, and, like it, directed downwards. The *coincidence of these* points with their images reflected in the mercury

surfaces, is observed with the aid of lenses of about 30 mm. focus, held in position upon the wooden framework of the apparatus. It is, of course, independent of any irregular refraction which the tube may exercise. The verticality of the line joining the points is tested without difficulty by a plumb-line.

The upper and lower chambers C, B are formed from tubing of the same diameter (about 21 mm. internal). The upper communicates through a tap, D, with the Töppler, by means of which a suitable vacuum can at any time be established and tested. In ordinary use, D stands permanently open, but its introduction was found useful in the preliminary arrangements and in testing for leaks. The connexion between the lower chamber B and the vessel in which the pressure is to be verified takes place through a side tube, E.

The greater part of the column of mercury to which the pressure is due is contained in the connecting tube FF, of about 3 mm. internal diameter. The temperature is taken by a thermometer whose bulb is situated near the middle of FF. Towards the close of operations the more sensitive parts are protected by a packing of tow or cotton-wool, held in position between two wooden boards. The anterior board is provided with a suitable glass window, through which the thermometer may be read.

It is an essential requirement of a manometer on the present plan that the measuring rod pass air-tight from the upper and lower chambers into the atmosphere. To effect this the glass tubing is drawn out until its internal diameter is not much greater than that of the rod. The joints are then made by short lengths of thick-walled india-rubber H,G, wired on and drowned externally in mercury. The vessels for holding the mercury are shown at I, K. There is usually no difficulty at all in making perfectly tight joints between glass tubes in this manner; but in the present case some trouble was experienced in consequence apparently of imperfect approximation between the *iron* and the mercury. At one time it was found necessary to supplement the mercury with vaseline. When tightness is once obtained, there seems to be no tendency to deterioration, and the condition of things is under constant observation by means of the Töppler.

The distance between the points of the rod is determined under microscopes by comparison with a standard scale, before the apparatus is put together. As the rod is held only by the rubber connexions, there is no fear of its length being altered by stress.

The adjustment of the mercury (distilled in a vacuum) to the right level is effected by means of the tube of black rubber LM, terminating in the reservoir N. When the supply of mercury to the manometer is a little short of what is needed, the connexion with the *reservoir is cut off* by a pinch-cock at O, and the fine adjustment

is continued by squeezing the tube at P between a pair of hinged boards, gradually approximated by a screw. This plan, though apparently rough, worked perfectly, leaving nothing to be desired.

It remains to explain the object of the vessel shown at Q. In the early trials, when the rubber tube was connected directly to R, the gradual fouling of the mercury surface, which it seems impossible to avoid, threatened to interfere with the setting at B. By means of Q, the mercury can be discharged from the measuring chambers, and a fresh surface constituted at B as well as at C.

The manometer above described was constructed by my assistant, Mr. Gordon, at a nominal cost for materials; and it is thought that the same principle may be applied with advantage in other investigations. In cases where a certain latitude in respect of pressure is necessary, the measuring rod might be constructed in two portions, sliding upon one another. Probably a range of a few millimetres could be obtained without interfering with the india-rubber connexions.

The length of the iron rod was obtained by comparison under microscopes with a standard bar R divided into millimetres. In terms of R the length at 15° C. is 762·248 mm. It remains to reduce to standard millimetres. Mr. Chaney has been good enough to make a comparison between R and the iridio-platinum standard metre, 1890, of the Board of Trade. From this it appears that the metre bar R is at 15° C. 0·3454 mm. too long; so that the true distance between the measuring points of the iron rod is at 15° C.

$$762{\cdot}248 \times 1{\cdot}0003454 = 762{\cdot}511 \text{ mm.}$$

Connexions with Pump and Manometer.

Some of the details of the process of filling the globe with gas under standard conditions will be best described later under the head of the particular gas; but the general arrangement and the connexions with the pump and the manometer are common to all. They are sketched in fig. 2, in which S represents the globe, T the inverted bell-glass employed to contain the enveloping ice. The connexion with the rest of the apparatus is by a short tube U of thick rubber, carefully wired on. The tightness of these joints was always tested with the aid of the Töppler X, the tap V leading to the gas-generating apparatus being closed. The side tube at D leads to the vacuum chamber of the manometer, while that at E leads to the pressure chamber B. The wash-out of the tubes, and in some cases of the generator, was aided by the Töppler. When this operation was judged to be complete, V was again closed, and a good vacuum made in the parts still connected to the pump. W would then be *closed, and the actual filling* commenced by opening V, and finally

Fig. 2.
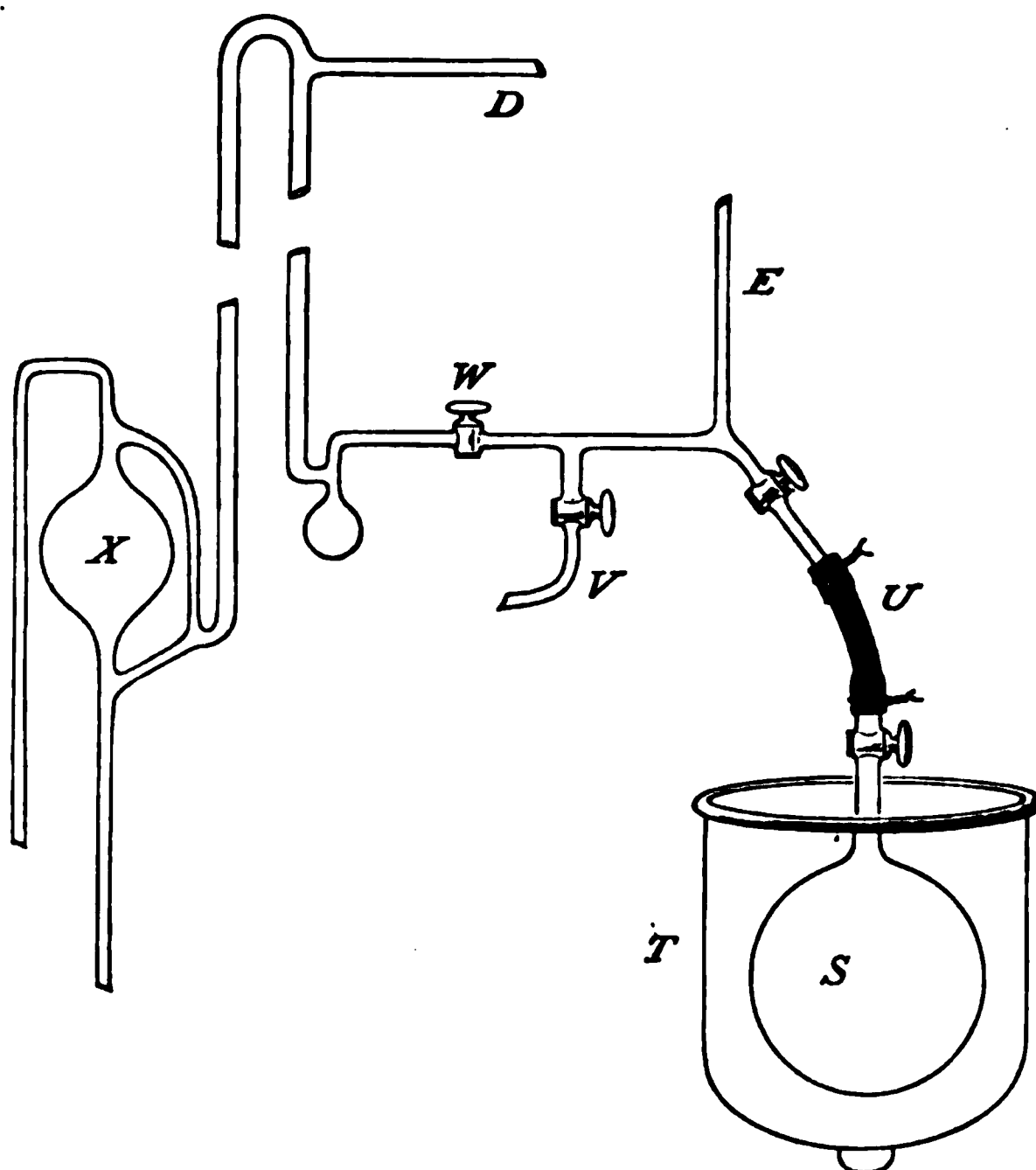
the tap of the globe. The lower chamber of the manometer was now in connexion with the globe, and through a regulating tap (not shown) with the gas-generating apparatus. By means of the Töppler the vacuum in the manometer could be carried to any desired point. But with respect to this a remark must be made. It is a feature of the method employed* that the exhaustions of the globe are carried to such a point that the weight of the residual gas may be neglected, thus eliminating errors due to a second manometer reading. There is no difficulty in attaining this result, but the delicacy of the Töppler employed as a gauge is so great that the residual gas still admits of tolerably accurate measurement. Now in exhausting the head of the manometer it would be easy to carry the process to a point much in excess of what is necessary in the case of the globe, but there is evidently no advantage in so doing. The best results will be obtained by carrying both exhaustions to the *same* degree of perfection.

At the close of the filling the pressure has to be adjusted to an exact value, and it might appear that the double adjustment required (of pressure and of mercury) would be troublesome. Such was not

* Due to von Jolly.

found to be the case. After a little practice the manometer could be set satisfactorily without too great a delay. When the pressure was nearly sufficient, the regulating tap was closed, and equilibrium allowed to establish itself. If more gas was then required, the tap could be opened momentarily. The later adjustments were effected by the application of heat or cold to parts of the connecting tubes. At the close, advantage was taken of the gradual rise in the temperature which was usually met with. The pressure being just short of what was required, and V being closed, it was only necessary to wait until the point was reached. In no case was a reading considered satisfactory when the pressure was changing at other than a very slow rate. It is believed that the comparison between the state of things at the top and at the bottom of the manometer could be effected with very great accuracy, and this is all that the method requires. At the moment when the pressure was judged to be right, the tap of the globe was turned, and the temperature of the manometer was read. The vacuum was then verified by the Töppler.

The Weights.

The object of the investigation being to ascertain the *ratio* of densities of water and of certain gases under given conditions, the absolute values of the weights employed is evidently a matter of indifference. This is a point which I think it desirable to emphasise, because v. Jolly, in his, in many respects, excellent work upon this subject,* attributes a discrepancy between his final result for oxygen and that of Regnault to a possible variation in the standard of weight. On the same ground we may omit to allow for the buoyancy of the weights as used in air, since only the *variations* of buoyancy, due, for example, to changing barometer, could enter; and these affect the result so little that they may safely be neglected.†

But, while the absolute values of the weights are of no consequence, their relative values must be known with great precision. The investigation of these over the large range required (from a kilogramme to a centigramme) is a laborious matter, but it presents nothing special for remark. The weights quoted in this paper are, in all cases, corrected, so as to give the results as they would have been obtained from a perfectly adjusted system.

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* 'Munich Acad. Trans.,' vol. 13, Part II, p. 49, 1880.

† In v. Jolly's calculations the buoyancy of the weights seems to be allowed for in dealing with the water, and neglected in dealing with the gases. If this be so, the result would be affected with a slight error, which, however, far exceeds any *that could arise from neglecting* buoyancy altogether.

The Water Contents of the Globe.

The globe being packed in finely-divided ice, was filled with boiled distilled water up to the level of the top of the channel through the plug of the tap, that is, being itself at 0°, was filled with water also at 0°. Thus charged the globe had now to be weighed; but this was a matter of some difficulty, owing to the very small capacity available above the tap. At about 9° there would be a risk of overflow. Of course the water could be retained by the addition of extra tubing, but this was a complication that it was desired to avoid. In February, 1882, during a frost, an opportunity was found to effect the weighing in a cold cellar at a temperature ranging from 4° to 7°. The weights required (on the same side of the balance as the globe and its supports) amounted to 0·1822 gram. On the other side were other weights whose values did not require to be known so long as they remained unmoved during the whole series of operations. Barometer (corrected) 758·9 mm.; temperature 6·3°.

A few days later the globe was discharged, dried, and replaced in the balance with tap open. 1834·1701 grams had now to be associated with it in order to obtain equilibrium. The difference,

$$1834·170 - 0·182 = 1833·988,$$

represents the weight of the water less that of the air displaced by it. The difference of atmospheric conditions was sufficiently small to allow the neglect of the *variation* in the buoyancy of the glass globe and of the brass counterpoises.

It remains to estimate the actual weight of the air displaced by the water under the above mentioned atmospheric conditions. It appears that, on this account, we are to add 2·314, thus obtaining

$$1836·30$$

as the weight of the water at 0° which fills the globe at 0°.

A further small correction is required to take account of the fact that the usual standard density is that of water at 4° and not at 0°. According to Broch (Everett's 'C.G.S. System of Units'), the factor required is 0·99988, so that we have

$$\frac{1836·30}{0·99988} = 1836·52$$

as the weight of water at 4° which would fill the globe at 0°.

Air.

Air drawn from outside (in the country) was passed through a solution of *potash*. On leaving the regulating tap it traversed tubes

filled with fragments of potash, and a long length of phosphoric anhydride, followed by a filter of glass wool. The arrangements beyond the regulating tap were the same for all the gases experimented upon. At the close of the filling it was necessary to use a condensing syringe in order to force the pressure up to the required point, but the air thus introduced would not reach the globe. It may be well to give the results for air in some detail, so as to enable the reader to form a judgment as to the degree of accuracy attained in the manipulations.

Date.	Globe empty.	Globe full.	Temp. of manometer.	Correction to 15°.	Corrected to 15°.
1892.					
Sept. 24	2·90941				
„ 27	..	0·53327	17·8	−0·00112	0·53219
„ 28	2·90867	..			
„ 29	..	0·53271	15·7	−0·00028	0·53243
Oct. 1	2·90923	..			
„ 3	..	0·53151	12·7	+0·00093	0·53244
„ 4	2·90872				
		Tap regreased.			
„ 7	2·91036				
„ 8	..	0·53296	12·4	+0·00105	0·53401
„ 10	2·91056				
„ 11	..	0·53251	11·8	+0·00129	0·53380
„ 12	2·91039				
„ 13	..	0·53201	11·0	+0·00161	0·53362
„ 14	2·91043				
„ 15	..	0·53219	10·6	+0·00177	0·53396

The column headed " globe empty " gives the (corrected) weights, on the side of the working globe, required for balance. The third column gives the corresponding weights when the globe was full of air, having been charged at 0° and up to the pressure required to bring the mercury in the manometer into contact with the two points of the measuring rod.

This pressure was not quite the same on different occasions, being subject to a temperature correction for the density of mercury and for the expansion of the iron rod. The correction is given in the fifth column, and the weights that would have been required, had the temperature been 15°, in the sixth. The numbers in the second and sixth columns should agree, but they are liable to a discontinuity when the tap is regreased.

In deducing the weight of the gas we compare each weighing "full" with the mean of the preceding and following weights

" empty," except in the case of October 15, when there was no sub-
sequent weighing empty.   The results are

September	27	2·37686
„	29	2·37651
October	3	2·37653
„	8	2·37646
„	11	2·37668
„	13	2·37679
„	15	2·37647
	Mean	2·37661

There is here no evidence of the variation in the density of air sus-
pected by Regnault and v. Jolly.   Even if we include the result for
September 27th, obviously affected by irregularity in the weights of
the globe empty, the extreme difference is only 0·4 milligram, or about
1/6000th part.

To allow for the contraction of the globe (No. 14) when weighed
empty, discussed in my former papers, we are to add 0·00056 to the
apparent weight, so that the result for air becomes

$$2·37717.$$

This is the weight of the contents at $0°$ and under the pressure
defined by the manometer gauge at $15°$ of the thermometer.   The
reduction to standard conditions is, for the present, postponed.

Oxygen.

This gas has been prepared by three distinct methods: (*a*) from
chlorates, (*b*) from permanganate of potash, (*c*) by electrolysis.

In the first method mixed chlorates of potash and soda were
employed, as recommended by Shenstone, the advantage lying in the
readier fusibility.   The fused mass was contained in a Florence flask,
and during the wash-out was allowed slowly to liberate gas into a
vacuum.   After all air had been expelled, the regulating tap was
closed, and the pressure allowed gradually to rise to that of the
atmosphere.   The temperature could then be pushed without fear of
distorting the glass, and the gas was drawn off through the regulating
tap.   A very close watch over the temperature was necessary to
prevent the evolution of gas from becoming too rapid.   In case of
excess, the superfluous gas was caused to blow off into the atmo-
sphere, rather than risk imperfect action of the potash and phosphoric
anhydride.   Two sets of five fillings were effected with this oxygen.
In the first set (May, 1892) the highest result was 2·6272, and the

lowest 2·6266, mean 2·62691. In the second set (June, July, 1892) the highest result was 2·6273 and the lowest 2·6267, mean 2·62693.

The second method (*b*) proved very convenient, the evolution of gas being under much better control than in the case of chlorates. The recrystallised salt was heated in a Florence flask, the wash-out, in this case also, being facilitated by a vacuum. Three fillings gave satisfactory results, the highest being 2·6273, the lowest 2·6270, and the mean 2·62714. The gas was quite free from smell.

By the third method I have not as many results as I could have wished, operations having been interrupted by the breakage of the electrolytic generator. This was, however, of less importance, as I had evidence from former work that there is no material difference between the oxgen from chlorates and that obtained by electrolysis. The gas was passed over hot copper, as detailed in previous papers. The result of one filling, with the apparatus as here described, was 2·6271. To this may be added the result of two fillings obtained at an earlier stage of the work, when the head of the manometer was exhausted by an independent Sprengel pump, instead of by the Töppler. The value then obtained was 2·6272. The results stand thus :—

Electrolysis (2), May, 1892	2·6272
„ (1) „	2·6271
Chlorates (5), May, 1892	2·6269
„ (5), June, 1892	2·6269
Permanganate (3), January, 1893 ..	2·6271
Mean	2·62704
Correction for contraction..	0·00056
	2·62760

It will be seen that the agreement between the different methods is very good, the differences, such as they are, having all the appearance of being accidental. Oxygen prepared by electrolysis is perhaps most in danger of being light (from contamination with hydrogen), and that from chlorates of being abnormally heavy.

Nitrogen.

This gas was prepared, in the usual manner, from air by removal of oxygen with heated copper. Precautions are required, in the first place, to secure a sufficient action of the reduced copper, and, secondly, as was shown by v. Jolly, and later by Leduc, to avoid contamination with hydrogen which may be liberated from the copper. I have followed the plan, recommended by v. Jolly, of causing the gas to pass finally over a length of unreduced copper. The arrangements were as follows :—

Air drawn through solution of potash was deprived of its oxygen by reduced copper, contained in a tube of hard glass heated by a large flame. It then traversed a ∪-tube, in which was deposited most of the water of combustion. The gas, practically free, as the event proved, from oxygen, was passed, as a further precaution, over a length of copper heated in a combustion furnace, then through strong sulphuric acid,* and afterwards back through the furnace over a length of oxide of copper. It then passed on to the regulating tap, and thence through the remainder of the apparatus, as already described. In no case did the copper in the furnace, even at the end where the gas entered, show any sign of losing its metallic appearance.

Three results, obtained in August, 1892, were—

August 8	....	2·31035
„ 10	....	2·31026
„ 15	....	2·31024
Mean..........		2·31028

To these may be added the results of two special experiments made to test the removal of hydrogen by the copper oxide. For this purpose a small hydrogen generator, which could be set in action by closing an external contact, was included between the two tubes of reduced copper, the gas being caused to bubble through the electrolytic liquid. The quantity of hydrogen liberated was calculated from the deflection of a galvanometer included in the circuit, and was sufficient, if retained, to alter the density very materially. Care was taken that the small stream of hydrogen should be uniform during the whole time (about $2\frac{1}{2}$ hours) occupied by the filling, but, as will be seen, the impurity was effectually removed by the copper oxide.† Two experiments gave—

September 17	....	2·31012
„ 20	....	2·31027
Mean..........		2·31020

We may take as the number for nitrogen—

	2·31026
Correction for contraction..	56
	2·31082

---

* There was no need for this, but the acid was in position for another purpose.

† Much larger quantities of hydrogen, sufficient to reduce the oxide over several centimetres, have been introduced without appreciably altering the weight of the gas.

Although the subject is not yet ripe for discussion, I cannot omit to notice here that nitrogen prepared from ammonia, and expected to be pure, turned out to be decidedly lighter than the above. When the oxygen of air is burned by excess of ammonia, the deficiency is about 1/1000th part.[*] When oxygen is substituted for air, so that all (instead of about one-seventh part) of the nitrogen is derived from ammonia, the deficiency of weight may amount to $\frac{1}{2}$ per cent. It seems certain that the abnormal lightness cannot be explained by contamination with hydrogen, or with ammonia, or with water, and everything suggests that the explanation is to be sought in a dissociated state of the nitrogen itself. Until the questions arising out of these observations are thoroughly cleared up, the above number for nitrogen must be received with a certain reserve. But it has not been thought necessary, on this account, to delay the presentation of the present paper, more especially as the method employed in preparing the nitrogen for which the results are recorded is that used by previous experimenters.

Reduction to Standard Pressure.

The pressure to which the numbers so far given relate is that due to 762·511 mm. of mercury at a temperature of 14·85°,[†] and under the gravity operative in my laboratory in latitude 51° 47′. In order to compare the results with those of other experimenters, it will be convenient to reduce them not only to 760 mm. of mercury pressure at 0°, but also to the value of gravity at Paris. The corrective factor for length is 760/762·511. In order to correct for temperature, we will employ the formula[‡] $1 + 0·0001818\,t + 0·00000000017\,t^2$ for the volume of mercury at $t°$. The factor of correction for temperature is thus 1·002700. For gravity we may employ the formula—

$$g = 980·6056 - 2·5028 \cos 2\lambda,$$

λ being the latitude. Thus, for my laboratory—

$$g = 981·193,$$

and for Paris—

$$g = 980·939,$$

the difference of elevation being negligible. The factor of correction is thus 0·99974.

The product of the three factors, corrective for length, for temperature, and for gravity, is accordingly 0·99914. Thus multiplied, the numbers are as follows :—

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[*] 'Nature,' vol. 46, p. 512.

[†] The thermometer employed with the manometer read 0·15° too high.

[‡] Everett, p. 142.

	Air.	Oxygen.	Nitrogen.
	2·37512	2·62534	2·30883

and these may now be compared with the water contents of the globe, viz., 1836·52.

The densities of the various gases under standard conditions, referred to that of distilled water at 4°, are thus :—

	Air.	Oxygen.	Nitrogen.
	0·00129327	0·00142952	0·00125718

With regard to hydrogen, we may calculate its density by means of the ratio of densities of oxygen and hydrogen formerly given by me, viz., 15·882.   Hence

	Hydrogen.
	0·000090009.

The following table shows the results arrived at by various experimenters.   Von Jolly did not examine hydrogen.   The numbers are multiplied by 1000 so as to exhibit the weights in grams per litre :—

	Air.	Oxygen.	Nitrogen.	Hydrogen.
Regnault, 1847	1·29319	1·42980	1·25617	0·08958
Corrected by Crafts	1·29349	1·43011	1·25647	0·08988
Von Jolly, 1880.........	1·29351	1·42939	1·25787	..
Ditto corrected	1·29383	1·42971	1·25819	..
Leduc, 1891*	1·29330	1·42910	1·25709	0·08985
Rayleigh, 1893	1·29327	1·42952	1·25718	0·09001

The correction of Regnault by Crafts† represents allowance for the contraction of Regnault's globe when exhausted, but the data were not obtained from the identical globe used by Regnault. In the fourth row I have introduced a similar correction to the results of von Jolly. This is merely an estimate founded upon the probability that the proportional contraction would be about the same as in my own case and in that of M. Leduc.

In taking a mean we may omit the uncorrected numbers, and also that obtained by Regnault for nitrogen, as there is reason to suppose that his gas was contaminated with hydrogen.   Thus

Mean Numbers.

Air.	Oxygen.	Nitrogen.	Hydrogen.
1·29347	1·42961	1·25749	0·08991

* 'Bulletin des Séances de la Société de Physique.'
† 'Comptes Rendus,' vol. 106, p. 1664.

L 2

The evaluation of the densities as compared with water is exposed to many sources of error which do not affect the comparison of one gas with another. It may therefore be instructive to exhibit the results of various workers referred to air as unity.

	Oxygen.	Nitrogen.	Hydrogen.
Regnault (corrected)	1·10562	0·97138	0·06949
v. Jolly (corrected)	1·10502	0·97245	
Leduc	1·1050	0·9720	0·06947
Rayleigh.................	1·10535	0·97209	0·06960
Mean	1·10525	0·97218	0·06952

As usually happens in such cases, the concordance of the numbers obtained by various experimenters is not so good as might be expected from the work of each taken separately. The most serious discrepancy is in the difficult case of hydrogen. M. Leduc suggests[*] that my number is too high on account of penetration of air through the blow-off tube (used to establish equilibrium of pressure with the atmosphere), which he reckons at 1 m. long and 1 cm. in diameter. In reality the length was about double, and the diameter one-half of these estimates; and the explanation is difficult to maintain, in view of the fact, recorded in my paper, that a prolongation of the time of contact from 4^m to 30^m had no appreciable ill effect. It must be admitted, however, that there is a certain presumption in favour of a lower number, unless it can be explained as due to an insufficient estimate for the correction for contraction. On account of the doubt as to the appropriate value of this correction, no great weight can be assigned to Regnault's number for hydrogen. If the atomic weight of oxygen be indeed 15·88, and the ratio of densities of oxygen and hydrogen be 15·90, as M. Leduc makes them, we should have to accept a much higher number for the ratio of volumes than that (2·0002) resulting from the very elaborate measurements of Morley. But while I write the information reaches me that Mr. A. Scott's recent work upon the volume ratio leads him to just such a higher ratio, viz., 2·00245, a number *a priori* more probable than 2·0002. Under the circumstances both the volume ratio and the density of hydrogen must be regarded as still uncertain to the 1/1000th part.

* 'Comptes Rendus,' July, 1892.

NOTE A.

On the Establishment of Equilibrium of Pressure in Two Vessels connected by a Constricted Channel.

It may be worth while to give explicitly the theory of this process, supposing that the difference of pressures is small throughout, and that the capacity of the channel may be neglected. If v_1, p_1 denote the volume and pressure of the gas in the first vessel at time t; v_2, p_2 the corresponding quantities for the second vessel, we have

$$v_1 \, dp_1/dt + c \, (p_1 - p_2) = 0,$$
$$v_2 \, dp_2/dt + c \, (p_2 - p_1) = 0,$$

where c is a constant which we may regard as the *conductivity* of the channel. In these equations inertia is neglected, only resistances of a viscous nature being regarded, as amply suffices for the practical problem. From the above we may at once deduce

$$\frac{d\,(p_1 - p_2)}{dt} + \left(\frac{c}{v_1} + \frac{c}{v_2} \right) (p_1 - p_2) = 0 \,;$$

showing that $(p_1 - p_2)$ varies as e^{-qt}, where

$$q = \frac{c}{v_1} + \frac{e}{v_2} = \frac{1}{\tau},$$

if τ be the time in which the difference of pressures is reduced in the ratio of $e : 1$.

Let us now apply this result (*a*) to the case where the globe of volume v_1 communicates with the atmosphere, (*b*) to the case where the globe is connected with a manometer of relatively small volume v_2. For (*a*) we have—

$$1/\tau = c/v_1,$$

and for (*b*)

$$1/\tau = c/v_2 \,;$$

so that

$$\tau/\tau' = v_1/v_2.$$

For such a manometer as is described in the text, the ratio v_1/v_2 is at least as high as 30; and in this proportion is diminished the time required for the establishment of equilibrium up to any standard of perfection that may be fixed upon.

III. "On the Variation of Surface Energy with Temperature."
By WILLIAM RAMSAY, Ph.D., F.R.S., and JOHN SHIELDS,
B.Sc., Ph.D. Received March 14, 1893.

(Abstract.)

It is shown that a close analogy exists between the equation for
gases,

$$pv = RT,$$

and an equation expressing the relation of surface energy to tempera-
ture,

$$\gamma s = \kappa \tau,$$

where γ is surface tension; s, surface; κ, a constant; and τ, tempe-
rature measured downwards from a point about 6° below the
critical point of the fluid. As the origin of T in the gaseous equation
is where $p = 0$, so the origin of τ should be where $\gamma = 0$. Correct-
ing the above equation so that τ shall represent the number of degrees
measured downwards from the critical point, the equation becomes

$$\gamma s = \kappa (\tau - d).$$

But even this equation does not express the whole truth. For at
temperatures less than 30° below the critical temperature, the
relation between surface energy and temperature is not a rectilinear
one; a correction is therefore introduced in the form of a second
term, which becomes insignificant at temperatures more than
25° or 30° τ; it is

$$\gamma s = \kappa \tau - \kappa d (1 - 10^{-\lambda \tau}).$$

The liquids examined were : ether, methyl formate, ethyl acetate,
carbon tetrachloride, benzene, chlorobenzene, acetic acid, and methyl
and ethyl alcohols; in fact, the only ones for which data are avail-
able. For, in order to calculate γ from the rise in a capillary tube,
it is necessary to know the density of the orthobaric liquid and gas;
and reliable data exist only for these liquids, and for a few others
which resemble them closely, *e.g.*, fluorobenzene, bromobenzene, &c.
Also to calculate s, *i.e.*, *molecular* surface, it is necessary to know the
molecular volume of the liquid, and to raise it to the power $\frac{2}{3}$rds.
Hence $v^{\frac{2}{3}} = s$, or molecular surface; *i.e.*, it is possible to compare
different liquids on the surfaces of which equal numbers of molecules
lie.

Measurements were made at $-89 \cdot 8°$, the boiling point of nitrous
oxide under atmospheric pressure, with ether, methyl formate, ethyl
acetate, and the two alcohols; the other substances are solid at that low

temperature.   These observations confirmed the rectilinear relation with the first three; but in the case of the two alcohols, evidence was obtained of molecular association, as also with acetic acid.   It is possible to calculate the amount of association at any temperature in such cases.   For, assuming the constancy of κ for the molecular surface of the "normal" liquids, the equation

$$\kappa/d = x^{\mathbf{1}},$$

where d is the differential coefficient of an associating liquid, and x is the molecular aggregation, gives the number of simple molecules which have united to form a compound at the temperature chosen. For the alcohols at $-90°$, and for acetic acid at $20°$, the association of molecules approximates to $(C_2H_4O_2)_4$, $(CH_4O)_4$, and $(C_2H_6O)_4$,

We have thus a method by which it is possible to ascertain the molecular complexity of undiluted liquids.   The results with the alcohols are shown to agree within reasonable limits with those obtained from strong solutions by Raoult's method.

It is incidentally shown in the course of the paper that there is no angle of contact between liquid and glass, when the liquid surface is in contact only with its own vapour.   Ordinary measurements of capillarity give inconstant, and probably inaccurate, results, for it is not the surface tension of the liquid which is measured, but that of a solution of air in the surface film of the liquid.

The paper contains tables and curves exemplifying and illustrating the statements given.

IV.   "The Absolute Thermal Conductivities of Copper and Iron."   By R. WALLACE STEWART, B.Sc. (Lond.), Assistant Lecturer and Demonstrator in Physics, University College, Bangor.   Communicated by LORD KELVIN, P.R.S. Received March 2, 1893.

(Abstract.)

The experiments described in the paper were undertaken with the object of determining the theoretical conductivity at different temperatures of iron, and, in particular, of pure electrolytically deposited copper.

The method adopted was that due to Forbes, with two modifications.

(a.) The thermo-electric method of determining temperature was employed.   The thermo-electric couple used was one of German silver and iron, giving, between 0° C. and 200° C., a uniform deflection on the galvanometer scale of about four divisions for a difference of one degree centigrade between the temperatures of its junctions.

(*b.*) The bar was protected from currents of air and external radiation by surrounding it by a trough of sheet zinc.

The range of temperature over which the observations extended was from 15° C. to about 220° C.

The iron bar used was a square ¾-inch bar of ordinary wrought iron; the copper bar was a round ½-inch bar of pure electrolytic copper. The reduction of the data of experiment was effected by the aid of curves drawn to a scale sufficiently large to secure the necessary accuracy.

In the case of the copper bar two distinct determinations, I and II, were made under different conditions, and the observations reduced separately. The results of these two determinations agreed within rather less than $1\frac{1}{2}$ per cent.

In order to reduce diffusivity to absolute conductivity, the densities of the iron and copper were determined hydrostatically, and the variation of the specific heat of iron with the temperature was determined by Bunsen's calorimeter with the result that the specific heat at $t°$ C. was found to be given by—$s_t = 0.1095(1 + 0.00008\,t)$. For the specific heat of copper the result given by Bède ($s_t = 0.0892 + 0.000065\,t$) was taken.

The final results obtained are indicated by the formulæ given below, and tend to show that for both copper and iron the conductivity decreases with rise of temperature.

Results for Iron in C.G.S. Units.

Diffusivity, κ, at $t°$ C. is given by—

$$\kappa_t = 0.208\,(1 - 0.00175\,t),$$

and the absolute thermal conductivity, k, by—

$$k_t = 0.172\,(1 - 0.0011\,t).$$

Results for Copper in C.G.S. Units.

Diffusivity, κ, at $t°$ C. is given by—

$$\text{I.} \quad \kappa_t = 1.370\,(1 - 0.00125\,t).$$
$$\text{II.} \quad \kappa_t = 1.391\,(1 - 0.00120\,t).$$

The mean of these results is taken as—

$$\kappa_t = 1.38\,(1 - 0.0012\,t),$$

and the value of the absolute conductivity, k, is then given by—

$$k_t = 1.10\,(1 - 0.00053\,t).$$

As the experimental observations supply data for the calculation of the emissive power of the surfaces of the bars at different temperatures, a table is given at the end of the paper showing the emissive power of the surface of each bar at temperatures between 20° C. and 200° C. The values obtained agree fairly with those given by Macfarlane and Tait for somewhat similar surfaces.

V. "Preliminary Notice on the Arrow-Poison of the Wa Nyika and other Tribes of East Equatorial Africa, with special reference to the Chemical Properties and Pharmacological Action of the Wood from which it is prepared." By THOMAS R. FRASER, M.D., F.R.S., Professor of Materia Medica in the University of Edinburgh, and JOSEPH TILLIE, M.D. (Edin.). Received March 6, 1893.

Burton,[*] Cameron,[†] and other travellers have given accounts of much interest of an arrow-poison used in warfare and in the chase by the Wa Nyika, Wa Kamba, Wa Gyriama, and other tribes of Eastern Equatorial Africa. The poison was stated to be prepared from the wood of the stem and root of a tree, which, however, was not botanically identified.

Several years ago, an opportunity was given to one of us to examine poisoned arrows, and the poison used in smearing them, of the Wa Nyika tribe. While the pharmacological action of this poison was found to have a close resemblance to that of *Strophanthus* seeds, its physical and chemical properties enabled the conclusions to be drawn that the poison was not made from these seeds, but was chiefly composed of an extract prepared from a wood.[‡]

These conclusions have been confirmed by the examination of further specimens of the Wa Nyika arrow-poison, and of the wood from which it is prepared. The specimens were most kindly sent to one of us, at various times between 1889 and 1892, by the Rev. William Morris, of the Church Missionary Society's East African Mission, and by Mr. Berkeley, the Administrator to the Imperial British East Africa Company at Mombasa.

These gentlemen have also sent the leaves and fruit of the plant, which have enabled us to identify it as an *Acokanthera*; but, as flowers have not yet been obtained, it has not been possible to determine the species.

[*] 'The Lake Regions of Central Africa,' 1860, vol. 2, p. 305.
[†] 'Across Africa,' 1885, p. 59.
[‡] Fraser, "On *Strophanthus hispidus*: its Natural History, Chemistry, and Pharmacology," 'Edinburgh Roy. Soc. Trans.,' vol. 35, Part IV, 1890, pp. 966–67.

In the present paper we propose to give a brief preliminary description of some of the most important of the results obtained by us in an extended examination of the chemical and pharmacological properties of the arrow-poison, and especially of the wood from which it is prepared, reserving fuller details until the flowers of the plant have been obtained.

We have found that the arrow-poison contains a crystalline glucosidal active principle, which in its chemical properties and pharmacological actions is identical with an active principle present in the wood, thus confirming the statement of the source of the poison.

One early and small supply of the wood did not yield a crystalline principle when the extract was treated by the tannin and oxide of lead process, and the limited supply at our disposal prevented the adoption of the process which, when applied to subsequently received supplies, led to the separation of the active principle in a crystalline form.

This process consists in the preparation of an alcoholic extract of the wood, the treatment of this with water, and the evaporation of the filtered watery solution. Impure crystals appear in the concentrated fluid, and their purification is effected by digestion of a hot alcoholic solution with charcoal, and subsequent recrystallisations from rectified spirit.

Thus obtained, the active principle occurs in the form of colourless thin needle-shaped crystals, which usually group themselves in tufts and rosettes. When crystallised from water it has the form of quadrangular plates.

At a temperature between 55° and 60° F. the crystals are soluble to the extent of about 0·93 per cent. in distilled water; of 0·41 per cent. in absolute alcohol; of 0·45 per cent. in diluted alcohol of sp. gr. 0·838; of 2·4 per cent. in diluted alcohol of sp. gr. 0·920. They are less soluble in acetone, amylic alcohol, and petroleum ether; and are altogether insoluble in ethylic ether and chloroform. Much larger quantities are dissolved by hot than by cold water and alcohol.

Ether, chloroform, and petroleum ether precipitate the active principle in a crystalline form from solutions in strong and dilute alcohol.

A saturated solution in cold water is tasteless and neutral in reaction; and it is obviously affected by very few chemical reagents, including the ordinary reagents for alkaloids. Silver nitrate and mercurous nitrate, however, produce white precipitates. Tannin does not cause any change in saturated cold water solutions, but it throws down a copious white precipitate in cold solutions, prepared by saturating water at the boiling temperature, and this precipitate is *soluble in an excess of the reagent and in water.*

When to the crystals themselves a little strong sulphuric acid is added, a pink colour is 'almost immediately developed, which soon darkens to a brick-red, and then slowly fades to pale brown. Dilute sulphuric acid, with moderate heat, changes the colourless crystals rapidly to brick-red, and then gradually chocolate and green colours are developed.

The exact melting point is not easily ascertained. When heated to about 184° C., the crystals suddenly almost disappear, and the soft substance remaining undergoes little further change until a temperature of rather over 200° C. is reached, when the colour becomes brown, and bubbles of gas are liberated.

No nitrogen or inorganic matter is present in the crystals. When they are heated at 100° C. in 2 per cent. sulphuric acid, a brownish amorphous substance is deposited, and the neutralised filtrate causes an abundant reduction in Fehling's solution, showing that the active principle is a glucoside.

Two concordant combustions made for us by Dr. Dobbin, of the Chemical Laboratory of the University, indicate that the probable formula of the substance is $C_{30}H_{52}O_{14}$.

These characters show that this active principle closely resembles, if it be not identical with, a crystalline substance separated by Arnaud,[*] by a more complicated process than the above, from the wood of a plant obtained in the Somali country; and, although the species has not yet been identified, the plant has, from an examination of the twigs and wood, been placed in the genus *Acokanthera*.[†]

On testing the pharmacological activity of the crystalline active principle obtained by us from the *Acokanthera* wood, we found that the minimum lethal dose for the frog (*Rana temporaria*) was between 0·00004 and 0·00005 grain per 100 grains of weight of frog. The latter dose always caused death, usually in from three to six hours; the former dose was not lethal. Rabbits died usually in a little over an hour after the subcutaneous administration of from 0·00003 to 0·000035 grain per 100 grains (1/400th to 1/500th grain per pound) of weight of rabbit.

The arrow-poison itself was found to have only one-fourth the lethal power of the crystalline active principle.

In a series of experiments on frogs and rabbits we found certain effects to occur so uniformly that they may be regarded as characteristic of the action of the poison.

In the frog the prominent effects which follow the subcutaneous injection of large lethal doses are:—Slowing and intermittence of respiration; gaping of the mouth, often accompanied with straining

* 'Comptes Rendus,' vol. 106, 1888, p. 1011, and vol. 107, 1889, p. 1162.

† *Ibid.*, vol. 107, 1889, p. 1162, and 'Bulletin Gén. de Thérap.,' vol. 117, 1889, p. 107.

movements like those of vomiting; fibrillary twitching of muscles, especially at the seat of injection; impairment of motor power and of co-ordination; disappearance of the cardiac impact; cessation of respiration; and gradual enfeeblement and loss of reflex and voluntary movement.

On opening the thorax, the heart is found motionless, the ventricle in extreme systole, the auricles distended with blood, and the whole heart inexcitable to mechanical or electric stimulation. When, however, the precise minimum lethal dose has been administered, the heart is found to have been arrested in extreme diastole, and it responds to stimulation. Immediately after death, stimulation of a motor nerve causes muscular contraction; but, soon thereafter, the muscles cease to respond to stimulation of the nerves or to direct irritation, and become acid in reaction and rigid.

In the rabbit, after the subcutaneous administration of a minimum lethal dose, the most important phenomena are gradual impairment and failure of the heart's action, of respiration, and of motor power. Just before death the cardiac pulsations become slow and extremely feeble, but the rate is estimated with difficulty by palpation of the thorax on account of frequent muscular twitchings. The respiration is rendered slow, irregular, and shallow. Inspiratory difficulty occasionally becomes so great that death from asphyxia seems impending even when the cardiac pulsations and general motor power appear good. Motor power is usually so much reduced before death that the animal lies prostrate; and only a few feeble movements of the body indicate the arrest of the heart. When the dose is large, or when the poison acts with unusual rapidity, the heart is paralysed before the general motor depression has set in, and sharp convulsive movements follow the arrest of the circulation. It is sometimes difficult to say whether the cardiac or the respiratory movements cease first. Usually, respiration is distinctly continued for a brief period after the heart can no longer be felt to pulsate, and on *post-mortem* examination, the bright red colour of the left auricle and the pulmonary veins contrasts strongly with the dark colour of the right auricle. Sometimes cardiac movements occur after the respiration has finally ceased, but immediate *post-mortem* examination reveals that the pulsations are mere irregular movements, altogether insufficient to sustain life or to indicate that the arrest of respiration was the cause of death. The left ventricle is usually found contracted and nearly empty, and the right ventricle and the auricles filled with blood. Most frequently the heart is motionless, and does not respond to mechanical or electrical stimulation, but it sometimes shows spontaneous quivering movements, and, for a very brief period, may respond to stimulation. The lungs are of a light pink colour. After death, the motor nerves soon lose all influence over

the muscles, and, in a very brief period thereafter, the muscles become inexcitable, acid in reaction, and rigid.

On analysing the action of the active principle we have found that, in the frog, the slowing, irregularity, and cessation of respiration, and the gaping movements of the mouth are not necessarily primary actions, but may be secondary to the arrest of the circulation, because control frogs, whose circulation has been mechanically arrested, exhibit these phenomena within a similar period of time. In rabbits, artificial respiration does not prevent death from cardiac failure ; but the impairment of respiration probably contributes in them to the fatal result in the case of doses bordering upon the minimum lethal.

The fibrillary muscular twitchings which occur in rabbits as well as in frogs are due to a primary action upon the endings of motor nerves.

The disappearance of reflex and voluntary movements after the administration of small lethal doses is due to paralysis of the nerve centres, and not to a peripheral action, for when one part of the body is protected, reflex and voluntary movements cease in the protected and unprotected parts at the same time. This central paralysis, however, is almost entirely due to the failure of the circulation, and resembles the paralysis in unpoisoned control frogs whose circulation is arrested. When large doses of the poison are administered subcutaneously to frogs, the depression of reflexes is partly due to peripheral causes, because, when one part is protected, that part exhibits more rapid and vigorous reflexes than the unprotected parts. This difference is largely due to a paralysing action on the muscles. It may, however, be partly caused by depression of sensibility, for when the action is limited to one part, stimulation of the skin in the poisoned area fails, after a time, to cause reflexes in the unpoisoned parts, although stimulation of a poisoned nerve trunk still excites reflex movements. The action of large doses upon the sensory nerves is well shown by applying 1/100th of a grain in solution to the cornea of the rabbit, when anæsthesia, lasting for several hours, along with some contraction of the pupil, is produced.

The motor nerves retain their influence upon the muscles until the latter show distinct signs of poisoning; but the muscles still react to strong, although not to moderate, electrical stimulation after stimulation of their motor nerves is no longer able to excite contractions.

The action on the heart is very pronounced. When a large dose is injected subcutaneously in the frog, or applied directly to the heart, the pulsations become slow owing to a great increase in the duration of the systole. Unequal contraction and relaxation of parts of the ventricular wall occur, the diastolic expansion becomes less and less, and, within twenty minutes after poisoning, the ventricle is arrested

in extreme and permanent systole. The auricles contract for a short time longer, but cannot empty themselves, and become arrested in a dilated state. The heart no longer responds to stimulation, and the muscle of the ventricle quickly acquires an acid reaction. After arrest of the heart, respiration may continue irregularly for so long as an hour, and for a time the frog can jump about actively.

The action of small doses upon the heart is, in several respects, essentially different from that of large doses. Several hours after the administration of the precise minimum lethal dose, the cardiac pulsations become very slow. The slowing, however, is not due to a lengthening of the systole, but to a great prolongation of the diastole, and of the succeeding pause in the heart's action. Gradually, periods of standstill occur in extreme diastole, and, when the heart spontaneously resumes beating, one or more auricular contractions precede those of the ventricle. The systolic contraction is extremely powerful. The condition of diastolic arrest often lasts many minutes at a time, and finally spontaneous pulsations cease. At this stage, the dilated heart responds, by one or more contractions, to any form of stimulation, but, if the stimulation be frequently repeated, the relaxation after each contraction becomes less and less, and the ventricle slowly passes into moderate, but permanent, systole. During these events, the inhibitory function of the vagus is not only retained, but increased. The diastolic arrest of the heart is not dependent upon "inhibition," however, for the condition is neither removed nor prevented by the administration of atropine. The accelerator action of the vagus is retained. The diastolic arrest is apparently due, therefore, to a direct action of the poison upon the motor ganglia and muscle of the heart.

The action upon the blood-vessels was found to be very slight. One part of the crystalline active principle in 10,000 parts of normal saline solution (0·75 per cent.) produced, when circulated through the vessels of a pithed frog, about the same effect as the pure saline solution, whereas 1 part of Merck's purest digitalin (soluble in water) in 50,000 parts of normal saline solution produced an extreme and rapid reduction in the calibre of the vessels. This difference in action upon the vessels is much accentuated by the fact that the lethal power of the digitalin in frogs was found to be only about 1/50th of that of the active principle of the *Acokanthera* wood.

In blood pressure experiments upon rabbits, the repeated administration of small non-lethal doses by injection into the jugular vein produced a remarkable slowing of the pulse, the vertical height of each pulse curve indicating at the same time a great increase in the force of the ventricular contraction. The blood pressure was usually found not to be increased, and, when increased, was evidently not the cause of the slow pulse. When rise of blood pressure from asphyxia

was guarded against by carrying on artificial respiration, the pressure was little affected, except when a marked fall occurred before death. The inhibitory action of the vagus was found to be retained, and the nerve proved to be intimately concerned in the early slow pulse, because the division of the vagi or the administration of atropine produced an immediate change in the tracing, the pulse becoming rapid and the movements relatively small. The further administration of the active principle restored to only some extent the original character of the tracing. When large doses were administered, the original slow pulse quickly became rapid and irregular, the blood pressure rose somewhat, and the respiration became disordered. The pressure then rapidly fell, and the cardiac pulsations became slow, intermittent, and feeble, and finally ceased before the pressure was at zero.

The action upon the circulatory, muscular, and nervous systems, therefore, closely resembles, if it be not identical with, that of strophanthin.

Note.

In 1880, an arrow-poison used by the Wa Nyika and Wa Kamba tribes was examined chemically by Gerrard,* and a non-crystalline substance, giving the reaction of a glucoside, was separated, and was found by Ringer* to be a powerful muscle-poison, which caused death by arresting the heart in systole. In 1887, Laborde† examined some features of the physiological action of a Wa Kamba arrow-poison, obtained from a missionary, M. A. Le Roy, and stated by him to be composed of parts of eight plants. Laborde found that the poison caused death by arresting the respiration and heart, and he came to the conclusion that the primary and predominant action was exercised upon the cardio-respiratory centres in the medulla.

In 1888, MM. Langlois and Varigny‡ examined the action of poisoned arrows obtained from the Somali country, and found that the poison caused arrest of respiration and of the heart, which they attributed to paralysis of the medullary centres.

In the same year, MM. Gley et Rondeau,§ and also M. Gley‖ separately, examined some points in the action of ouabaïn, the active principle separated by Arnaud from wood believed to be the source of the Somali arrow-poison, and concluded that the effects produced were due essentially to an action upon the medullary centres. Dr. Sailer,¶ in 1891, after an extended examination of the actions of

* 'Pharm. Journal and Trans.,' 1880–81, p. 835.

† 'Comptes Rend. de la Soc. de Biol.,' 1887, vol. 4, pp. 52, 370.

‡ *Ibid.*, 1888, vol. 5, p. 419.

§ *Ibid.*, p. 421.

‖ 'Comptes Rendus,' 1888, vol. 107, p. 348.

¶ 'Therapeutic Gazette,' 1891, vol. 15, pp. 727, 814.

ouabaïn, arrived at conclusions which are not in accordance with those of the French observers, viz. :—that the cardio-respiratory centres in the medulla are not primarily affected, that the lethal action of the poison is exercised directly upon the heart, and that the asphyxia is a secondary phenomenon.

The Society then adjourned over the Easter Recess to Thursday, April 20.

Presents, March 23, 1893.

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Second Report to the Royal Society Water Research Committee. The Vitality and Virulence of Bacillus anthracis *and its Spores in Potable Waters.*

By Percy F. Frankland, Ph.D., B.Sc., F.R.S., Professor of Chemistry in University College, Dundee, and H. Marshall Ward, D.Sc., F.R.S., F.L.S., Professor of Botany, Royal Indian Engineering College, Coopers Hill. Presented to the President and Council, March 16, 1893.

Introduction.

In our First Report we endeavoured to give a concise account of the more important work which has been done on the bacteriology of water, and devoted special attention to those investigations which have thrown any light on the vitality of pathogenic bacteria when submerged in aqueous media of various kinds and under various circumstances. We showed what a very large amount of scientific labour has already been bestowed on this comparatively new subject; and we as far as possible sifted this accumulated material, collecting those facts which may be reasonably regarded as definitely proved, and separating them from those which are more or less uncertain, problematical, or contradictory. The results of this critical analysis we embodied in a number of conclusions, to be found on pp. 124—219 of our Report, and to which we would again refer the reader.

In this summary and conclusions we have prominently pointed out that the information which is at present most urgently required from a practical point of view is with regard to the manner in which the vitality of pathogenic bacteria is affected by the presence of non-pathogenic or saprophytic forms. Although several investigators have already approached this question, we were obliged to indicate that their results have to be received with much caution, in consequence of the great experimental difficulties which attach to this inquiry, and which we were of opinion had not in some cases been adequately taken into account to prevent misleading results being obtained.

It was owing to our consciousness of these experimental difficulties that we resolved to confine our own investigations in the first instance to the study of such a pathogenic form as would permit of these difficulties being reduced to a minimum, and to this end we naturally selected the *Bacillus anthracis.* But in so doing we also had a second object in view, for, inasmuch as the spores of this bacillus are among the hardiest forms of pathogenic organisms, their deportment under any particular conditions is of peculiar interest as exhibiting the limit of endurance which has to be taken into consideration in dealing with pathogenic bacteria. Thus, conditions which are found to be fatal to anthrax spores may in general be assumed to be *a fortiori* fatal to other pathogenic forms. Again, we have in the case of anthrax the possibility of determining in a much more decisive manner than with other forms the influence which conditions exert on the virulence of the organism.

The Vitality of Bacillus anthracis *and its Spores in Water.*

As indicated in our First Report, this question has already occupied the attention of a number of investigators. In some cases the bacilli* free from spores, in other cases sporiferous bacilli, have been employed; again, in some experiments sterile, and in others unsterilised, waters have been used, whilst the temperature at which the waters were maintained during the experiments has also been varied.

Thus, Wolffhügel and Riedel ('Arbeiten a. d. Kais. Gesundheitsamte,' vol. 1, 1886, p. 455) introduced the bacilli, which may possibly have contained spores,† as they were taken from a gelatine culture, into sterile water kept at 35° C., and obtained abundant multiplication, whilst when similar bacilli were placed in water at 7—10° C. their presence was no longer demonstrable by culture after two days, although a few bacilli (or spores) must still have been present in a virulent condition, as of four mice, each inoculated with ⅓ c.c. of the water, one died of anthrax. The water employed in these experiments was primarily the polluted liquid found in the River Panke at Berlin, although the results were substantially similar when this water was diluted with ten times its volume of distilled water. These investigators made no experiments with unsterilised water.

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* We adopt the following terminology throughout:—*Asporogenous* means incapable of developing spores; *sporiferous bacilli* are bacilli actually containing spores, whereas *vegetative bacilli* are bacilli free of spores, though not necessarily incapable of developing them later.

† Gelatine cultures do not develop spores very rapidly, so that it is by no means certain that spores were introduced; however, these observers added so much gelatine that we cannot attach much value to the conclusions.

We may with advantage quote their experiments in full in the following tables (pp. 167 and 168).

Their results are in nearly all important respects entirely out of harmony with those of Meade Bolton, to whose experiments we may in the next instance refer.

Meade Bolton ('Zeitsch. f. Hygiene,' vol. 1, 1886, p. 76) found that on introducing anthrax bacilli presumed to be devoid of spores into ordinary drinking water (Göttingen water supply) sterilised by steam and kept ac 20° C., they were no longer demonstrable by culture after 6 days, whilst at 35° C. the same result was obtained in 55 hours. On the other hand, when he employed sporiferous anthrax bacilli, they were still demonstrable after 90 days at 20° C., both in sterile distilled and in sterile well water of bad quality, whilst at 35° C. in the same waters the organisms disappeared between the 30th and the 90th day as tested by cultivation. The waters were not tested for virulence by inoculating animals, nor does it appear whether or no in these latter experiments there was any multiplication of the anthrax bacilli, as an uncountable number were in the first instance introduced into the water. It will be most convenient to quote Meade Bolton's tabulated results *in extenso* (see p. 169).

Results in substantial agreement with those of Meade Bolton have also been recorded by Koch (Gärtner-Tiemann's book, p. 585) and Naegeli, who both state that the spores of anthrax preserve their vitality in distilled water for upwards of one year. Hochstetter ('Arbeiten a. d. Kais. Gesundheitsamte,' vol. 2, p. 1), again, found the anthrax bacilli free from spores to persist both in sterilised distilled and in sterilised drinking water for 3 days only at the outside (in some cases they actually disappeared in a quarter of an hour), whilst the sporiferous bacilli were still alive and virulent after 154 days in the same waters as well as in unsterilised seltzer water. (For detailed table of results, see pp. 170 and 171.)

Similarly again Hueppe ('Journ. f. Gasbeleucht.,' 1887, p. 129) found the anthrax bacilli no longer demonstrable on the fifth day in sterilised drinking water kept at 16° C.

Straus and Dubarry ('Arch. de Méd. Expér.,' 1889; 'Ann. de l'Inst. Pasteur,' vol. 4, pp. 109—124), on the other hand, found even the bacilli free from spores to retain their vitality in sterile drinking water at 20° C. for 28 days in one case and 65 days in another, and they proved that when such sporeless bacilli are introduced into distilled water they can form spores and persist for upwards of 131 days. The greater longevity of the anthrax bacilli in the hands of these investigators is doubtless due to their having employed a more sensitive method of cultivation than the others to whom we have referred, for, instead of simply submitting the waters to plate cultivation in the ordinary way, they first added broth to the water, so as to en-

Wolffhügel and Riedel's Experiments on the Vitality of Anthrax in Water. February, 1885.

Description.	Source of material.	Temperature in Centigrade degrees at which kept.	Volume of water used for cultivation.	Number of colonies found on gelatine plates prepared from the water infected.						
			c.c.	Directly after.	1 day.	2 days.	8 days.	4 days.	10 days.	15 days.
River Panke, unfiltered, 10 c.c.	Anthrax from gelatine culture, 1 needle-loop	85	$\frac{1}{100}$	420	8,000	84,800	..	..	..	..
,, ,, ,,	Anthrax from gelatine culture, 1 needle-point	85	$\frac{6}{100}$	0	4	..	4,900	..	5,600	..
River Panke, unfiltered, 10 c.c. diluted with distilled water (1 : 10)	Anthrax from gelatine culture, 1 needle-loop	16	$\frac{1}{100}$	800	4,000	12,500	..	..	4,800	..
River Panke, filtered, 10 c.c.	Anthrax from gelatine culture, 1 needle-point	85	$\frac{4}{100}$	0	0	..	1,040	2000	..	7,560
River Panke, filtered, 10 c.c., diluted with distilled water (1 : 1)	Anthrax from gelatine culture, 1 needle-point	-16	$\frac{1}{100}$	0	0	..	..	850	..	1,550

Wolffhügel and Riedel's Experiments on the Vitality of Anthrax in Water.   February, 1885.

Description.	Source of material.	Tempera-ture in Centigrade degrees at which kept.	Volume of water used for cultivation.	Number of micro-organisms found on gelatine plates prepared from the infected water.						
			c.c.	Directly after.	1 day.	2 days.	3 days.	4 days.	10 days.	15 days.
River Panke, unfiltered, 10 c.c.	Anthrax from gelatine culture, 1 needle-point	35	$\frac{1}{100}$	42	0	12,600	..	..	..	..
" "	"		$\frac{1}{1000}$	..	..	..	..	..	..	..
" "	"	7—10	$\frac{1}{100}$	5	0	0	0	0	*	..
" "	"		$\frac{1}{1000}$	..	..	..	..	..	..	..
River Panke, unfiltered, 10 c.c., diluted with distilled water (1:10)	"	35	$\frac{1}{100}$	49	9	10,000	12,500	14,800	8,750	12,000
" "	"		$\frac{1}{1000}$	..	..	..	..	9,450	3,160	21,000
" "	"	7—10	$\frac{1}{100}$	92	0	0	0	0	*	..
" "	"		$\frac{1}{1000}$	..	..	..	..	..	..	..
River Panke, filtered, 10 c.c.	"	35	$\frac{1}{100}$	5	600	10,400	14,000	27,000	44,000	16,000
" "	"		$\frac{1}{1000}$	..	..	..	..	7,000	..	4,800
" "	"	7—10	$\frac{1}{100}$	4	1	3	0	0	*	..
" "	"		$\frac{1}{1000}$	..	..	..	..	..	..	..
River Panke, filtered, 10 c.c., diluted with distilled water (1:10)	"	35	$\frac{1}{100}$	6	410	11,700	11,300	15,000	36,000	10,000
" "	"		$\frac{1}{1000}$	..	..	..	..	6,800	8,000	2,500
" "	"	7—10	$\frac{1}{100}$	28	7	3	0	0	†	..
" "	"		$\frac{1}{1000}$	..	..	..	..	..	..	..

* On the 9th day ½ c.c. was subcutaneously injected into a mouse. It remained alive.
† On the 9th day ½ c.c. was subcutaneously injected into a mouse. It died after 4½ days.

Meade Bolton's Experiments on the Vitality of Anthrax in Water, 1886.

Nature of material.	Temperature in Centigrade degrees.	Number of colonies obtained from 1 c.c. of water.		
		Directly after inoculation.	Hours. 55	Days. 6
Sporeless Anthrax.				
In ordinary tap-water.........	+20° C.	7740	832	0
„ „ „	85° C.	..	..	0

Anthrax Spores. Number of Colonies obtained from 1 c.c. of Water.

Description of water.	Temperature in Centigrade degrees.	Directly after inoculation.	2—5 days.	5—10 days.	10—20 days.	20—30 days.	90 days.
Distilled water..........	+20	Innumerable	..	..	Innumerable	1680	..
Polluted well water (filtered)	„	„	..	..	„	Innumerable	Innumerable
Polluted well water (unfiltered)	„	„	..	..	„	„	Innumerable

Hochstetter's Experiments on the Vitality of Anthrax in various Waters.

Source of material.	Nature of the water used.	Temperature at which the water was preserved.	Number of flasks or bottles inoculated.	Number of hours or days after which the examination was made.	Table showing the number of times the flasks or bottles were examined.												Longest vital duration.		Shortest vital duration.	
					I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.	Hrs.	Days.	Hrs.	Days.
Anthrax Bacilli. Juices from the organs of a guinea pig, dead 4 days after inoculation with anthrax.	Seltzer water	14—18° C.	9	Hours	•½	•1													4	
				Days		•1	•3													
	Distilled water	"	9	Hours		•1													1	
				Days		•1														
Anthrax Bacilli. Juices from the organs of a guinea pig, killed after 2 days whilst suffering from very pronounced anthrax.	Seltzer water	18—20° C.	8	Hours	•½													1	4	
				Days		•1	•2													
	Berlin water supply	"	4	Hours	•½	•1	•3	•13										1		2
				Days	•16	•16														
	Distilled water	"	4	Hours		•½														1
				Days		•8	•8	•13									1			
Anthrax Bacilli. Juices from the organs of a guinea pig, dead 2 days after inoculation with anthrax.	Seltzer water	11—13° C.	6	Hours	1½	•2½	•1	•8½	•3	•7									2	
				Days	•¼	•2½	•1	•8	•3	•7							8			7
	Berlin water supply	"	5	Hours		•2½	•1	•8	•7											7
	Distilled water	"	5	Hours	½	•2½	•1	•3	•7								3			
				Days													3			

Anthrax Bacilli,

Juices from the organs of a guinea pig, dead 2 days after inoculation with anthrax.

Seltzer water 13—16° C.	3 {	Hours	1	.. .. *1	..	..	..	..	..	..	..	..	..	..
		Days	..	..	..	..	..	..	..	..	..	..	1	..
Berlin water	3 {	Hours	1	..	..	..	..	..	..	..	..	..	..	..
		Days	..	1	1	..	..	..	..	..	..	..	1	1
supply Distilled water	2 {	Hours	1	1	1	..	..	..	..	..	..	..	1	..
		Days	..	..	..	..	..	..	..	..	..	..	..	..

Anthrax Spores,

Potato cultivation inoculated from the juices of a guinea pig, dead 2 days after injection with anthrax. The culture was kept for 5 days in the incubator at a temperature of 30° C.

Seltzer water 13—18° C. (bottles II—VI) 18—20° C. (bottles VII, VIII)	8 {	Hours	..	1	7	14	21	287	64[h]	154[4]	..	..	154	..
		Days	4	..	..	..	..	..	..	..	..	..	..	..
Berlin water supply	8 {	Hours	..	1	7	14	21	28	64	154[10]	..	..	154	..
		Days	4	..	..	..	..	..	..	..	..	..	..	..
Distilled water	8 {	Hours	..	1	7	14	21	25	64	154[10]	..	..	154	..
		Days	4	..	..	..	..	..	..	..	..	..	..	..

Note.—Those figures preceded by an asterisk (*) indicate that at the time of investigation the organisms were dead.

1 A mouse inoculated from a colony off a gelatine plate poured from this water died of anthrax after 2 days.

2 „

3 Guinea pigs inoculated with 4 c.c. of this infected seltzer water, subcutaneously injected, died after three days of anthrax; moreover, guinea pigs inoculated from a colony off a gelatine plate poured from this water died after two days of anthrax.

4 Bottle was kept in a room at a temperature of 18—20° C.

5 Guinea pigs inoculated with 4 c.c. of this infected seltzer water, subcutaneously injected, remained healthy.

4	„	„	„	„	„	died after 3 days of anthrax.
7	„	„	„	„	„	2 „ „ „
8	„	„	„	„	„	2 „ „ „
9	„	„	„	„	„	2 „ „ „
	„	„	„	„	„	2 „ „ „
10	„	„	Berlin	„	„	2 „ „ „

N 3

courage the multiplication of any few anthrax bacilli that might still be present in the living state, and which would have escaped detection if the water had been directly plate-cultivated.

Gärtner (Gärtner-Tiemann's 'Untersuch. d. Wassers,' p. 588, Brunswick, 1889) again introduced anthrax bacilli into unsterilised drinking water at 12° C., and found that they had all disappeared on the sixth day (presumably culture tests only were employed).

Important experiments of a similar nature had previously been made by Kraus (' Archiv f. Hygiene,' vol. 6, p. 234) with unsterilised water at 10·5° C.; on introducing anthrax bacilli free from spores, he found them no longer recognisable by cultivation tests on the fourth day, as shown in the following table :—

Kraus's Experiments on the Vitality of Sporeless Anthrax in Unsterilised Waters.

Source of water.	Number of days after inoculation when examined.				
	1	2	4	8	130
	Number of anthrax bacilli found in 1 c.c. of water.				
Munich water supply.........	1,150	900	0	0	0
Well water, Munich..........	1,050	1,000	0	0	0
,, ,, ,, ...	1,180	850	0	0	0

A similar result was later obtained by Karlinski (' Archiv f. Hygiene,' 1889, pp. 113—127; ' Centralbl. f. Bakteriol.,' vol. 6, p. 139) in unsterilised drinking water at 8° C.; the anthrax bacilli free from spores were by him found to have disappeared on the third day.

Uffelmann (' Centralbl. f. Bakteriol.,' vol. 5, p. 89), on the other hand, introduced sporiferous anthrax into unsterilised drinking water at 12—20° C., and found that its vitality was preserved for upwards of three months.

The only experiments which have been made with British waters are those which were carried out by one of us (Percy Frankland, Society of Chem. Ind., 1887), in which sporiferous anthrax was introduced into sterile distilled water, sterile Grand Junction water (filtered Thames water), and sterile London sewage. In all cases the vitality of the anthrax was preserved for upwards of 61 days,

and in the sewage extensive multiplication of the organism actually took place, as will be seen from the following table (see p. 174).

If we endeavour to summarise the results obtained by these several investigators, the evidence would appear to point to the following conclusions:—

1. Spores of anthrax retain their vitality either in sterile or unsterilised waters of the most varied character for long periods of time, many months, at ordinary or low temperatures, whilst they are slowly destroyed if the waters are kept at 35° C.

2. The evidence concerning the sporeless anthrax bacilli is somewhat contradictory. Most observers agree that they are rapidly destroyed in a few days both in sterilised and unsterilised waters; Straus and Dubarry, however, using a more delicate method of cultivation, have found their vitality to be retained for a much longer period, viz., from 28 to 65 days. There can be little doubt that in these experiments spores were formed in the waters. The results of Wolffhügel and Riedel differ also from those of other observers, probably owing to their having added so much gelatine, inasmuch as they found the anthrax bacilli to undergo abundant multiplication in sterile waters at 16° and at 35° C., whilst rapid destruction took place if the same waters were maintained at 7—10° C.

3. As regards the power of anthrax to propagate in water, there is, with the exception of the last mentioned results of Wolffhügel and Riedel, no evidence that they undergo multiplication in ordinary potable waters even when sterilised; indeed, it has been clearly shown by one of us that no numerical increase takes place either in sterile distilled or sterile filtered Thames water (Grand Junction Company), whilst in sterile London sewage the numbers underwent very considerable multiplication. The power of propagation in sterile water at any rate is, therefore, dependent on its chemical composition. It should be mentioned that the multiplication observed by Wolffhügel and Riedel was not in potable water proper, but in the water of the River Panke at Berlin, which is, or was, practically diluted sewage, although they also obtained multiplication when this water was diluted with ten times its volume of distilled water. The whole question of multiplication, however, is doubtless to a large extent dependent on the vigour of the anthrax growths employed for experiment.

4. It is much to be regretted that so few investigators have made any experiments on the virulence of the anthrax organisms after their residence in waters under varied conditions; this is, after all, the chief point of practical importance, it is the point

Percy Frankland's Experiments on the Vitality of Sporiferous Anthrax in Water.

Sporiferous anthrax.	1st hour after infection.	2nd day.	5th day.	12th day.	21st day.	40th day.	61st day.
Distilled water—							
No. 1..........	69	72	58	70	..	67	110
No. 2..........	65	65	53	80	..	Contaminated with mould 89	36
No. 3..........	106	67	87	63	..	88	100
Grand Junction water—							
No. 1..........	340	230	67	81	..	75	100
No. 2..........	530	175	44	95	..	67	88
No. 3..........	392	187	54	68	..	Largely contaminated with small smooth-rimmed colony. 80	121
London sewage—							
No. 1..........	753	39	82	Much multiplied	2041	..	5543
No. 2..........	108	240	114	498 Probably more.	547	..	290
No. 3..........	289	145	129	Contaminated	..	..	..

which admits of the most ready determination, and it is quite unaccountable why in the experiments made on the Continent it should have been so frequently neglected.

Object and Nature of our Experiments.

One of our first objects has been to make ourselves acquainted with the nature of the waters, and especially that of the Thames, selected for investigation; and this not only as regards their chemical composition, but also as regards the nature and numbers of organisms normally found in the water. We have also made inquiries as to the changes the water undergoes on standing, and have acquired much interesting information regarding these points.

In pursuing the inquiry as to the vitality of anthrax in water, we have been guided by the following considerations. As already pointed out, we selected the *Bacillus anthracis* for the first series of our investigations on the vitality of pathogenic bacteria in water, because it constitutes almost the extreme term, so to speak, in the series of pathogenic organisms which are at present known. In the form of spores it presents one of the hardiest and most refractory examples of living organisms,* at any rate of the pathogenic kind, for amongst the non-pathogenic forms there are a number which excel it in this respect; whereas our information as regards the bacilli in water is most incomplete, though the whole practical interest as regards pathogenic organisms turns on their behaviour in the vegetative—spore-free—condition, and on whether they can multiply or develop spores in the water.

In introducing the anthrax bacilli and their spores into British waters of typical character, we have endeavoured to ascertain whether their fate is affected (*a*) by differences of temperature such as occur in the natural course of events, (*b*) by the other bacteria present in the water; and, in order to ascertain this point, we have employed the waters in question in their natural state, unsterilised; also sterilised by heat (steam) in the ordinary way; and, thirdly, sterilised without the application of heat by filtration through unglazed porcelain.

We have also endeavoured to ascertain whether the sporiferous anthrax bacilli are differently affected according as the water in which they are resident is kept in darkness, placed in diffused light, or exposed to direct sunshine.

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* Pasteur showed ('Compt. Rend.,' 1877, vol. 85, p. 99) that they remain alive for some time in absolute alcohol, and for twenty-one days exposed to a pressure of 10 atmos. of pure oxygen. Koch has shown ('Mitth. a. d. Kaiserl. Ges. Amt.,' I, p. 32) what extreme temperatures they will endure, and Klein declares that ten minutes' boiling cannot be relied on. We have already given the literature showing that the spores remain for long periods alive in water, and it is well known they can be kept intact for months on silk threads in the dark.

Finally, we have not merely confined ourselves to the ordinary cultivation tests for ascertaining the vitality or otherwise of the anthrax organism under these several conditions; but we have also submitted the virulence of the infected waters under investigation to the direct test of inoculation into animals.

Inasmuch as the investigation of the above points has been conducted by each of us to a certain extent independently of the other, it has been proposed to record our experiments separately also, as in this manner the course of the two investigations is most easily followed, and the results obtained most readily surveyed. In the following pages, therefore, will be found a separate account of the independent inquiry pursued by each of us, the report closing with a number of conclusions which we have together drawn from the experimental material collected by both.

PART I.

" Experiments on the Vitality and Virulence of Sporiferous Anthrax in Potable Waters." By Professor PERCY FRANK-LAND, Ph.D., F.R.S., assisted by J. R. APPLEYARD, F.C.S.

The first water selected for experiment was that of the River Thames, which may be taken as a type of a calcareous surface water draining from cultivated land, but receiving only such a moderate proportion of organic impurity as to leave it in a condition that, judged by the ordinary standards of taste, smell, and appearance to the eye, it is suitable for drinking purposes. This is in fact the water which has for years been supplied to the larger part of the metropolis, and is, therefore, in some respects the most interesting water, from a hygienic point of view, in the United Kingdom.

The second water experimented with is that of Loch Katrine as supplied to Glasgow, which again is typical of those upland surface waters, collected from almost entirely uninhabited areas, which have been so largely utilised during the past 30 years for the supply of the great manufacturing districts of the north. These waters, and notably that of Loch Katrine, are characterised by their great soft-ness and almost entire freedom from mineral matters, whilst the organic constituents are almost wholly of vegetable origin, and thus differ more qualitatively than quantitatively from those present in waters such as that of the Thames.

Experiments with Thames Water.

In order to render the experiments as comparable as possible they have all been made on one and the same sample of Thames water, which was collected personally by my colleague, Professor Marshall

Summary of the Monthly Reports on the Bacteriological Condition of the London Water-Supply presented to the Local Government Board by Percy F. Frankland in 1886.

Number of Colonies obtained from 1 c.c. of Water by Gelatine-plate Cultivation.

Name of supply.	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	Average for year.
THAMES.													
Thames water, un-filtered (Hampton)	45,000	15,800	11,415	12,250	4,800	8,300	8,000	6,100	8,400	8,600	56,000	68,000	..
Chelsea	159	305	299	94	59	60	59	303	87	34	65	222	..
West Middlesex	180	80	175	47	19	145	45	25	27	22	47	2,000	..
Southwark	2,270	284	1,562	77	29	94	380	60	49	61	321	1,100	..
Grand Junction	4,894	208	879	115	51	17	14	12	17	77	80	1,700	..
Lambeth	2,687	265	287	209	136	129	155	1,415	59	45	103	305	..
Reduction per cent...	95·6	98·6	95·3	99·1	98·8	98·9	95·6	94·0	99·4	99·4	99·8	98·3	97·6
LEE.													
Lee water, unfiltered (Chingford)	39,300	20,600	9,025	7,300	2,950	4,700	5,400	4,300	3,700	6,400	12,700	121,000	..
New River	863	74	95	60	22	53	46	55	17	10	32	400	..
East London	224	252	533	269	143	445	134	243	165	97	248	280	..
Reduction per cent.*	99·4	98·8	94·1	96·3	95·2	90·5	97·5	94·3	95·5	98·5	98·0	99·8	96·5
DEEP WELLS (Kent Company).													
Bath Well	..	..	..	..	..	..	..	..	..	..	12	10	..
Garden Well	..	5†	44†	7†	8†	4†	12†	9†	5†	8†	..	..	..
New Well	..	..	..	..	..	..	..	..	..	160†	..	11	..
Supply	43	149	38	47	101	89	48	13	25	{283 / 405}	196	66	..

* These reductions apply only to the East London supply.

† In all cases marked with a dagger the name of the particular well was not mentioned.

Summary of the Monthly Reports on the Bacteriological Condition of the London Water-Supply presented to the Local Government Board by Percy F. Frankland in 1887.

Number of Colonies obtained from 1 c.c. of Water by Gelatine-plate Cultivation.

Name of supply.	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	Average for year.
THAMES.													
Thames water, unfiltered (Hampton)	30,800	6,700	30,900	52,100	2,100	2,200	2,500	7,200	16,700	6,700	81,000	19,000	...
Chelsea	5,300	81	171	55	49	190	106	44	73	64	187	86	...
West Middlesex	258	27	96	110	82	123	40	87	82	28	53	113	...
Southwark	4,900	428	1,825	360	61	196	119	70	84	130	152	133	...
Grand Junction	7,500	612	443	109	48	103	35	78	15	80	55	80	...
Lambeth	1,200	188	884	103	53	521	108	733	85	96	1,120	196	...
Reduction per cent.	87·6	96·0	98·1	99·7	97·7	89·7	96·7	97·2	99·6	98·8	99·6	99·4	96·7
LEE.													
Lee water, unfiltered (Chingford)	37,700	7,900	24,000	1,330	2,200	12,200	12,300	5,300	9,200	7,600	27,000	11,000	...
New River	508	72	133	38	16	31	83	15	23	25	41	39	...
East London	6,700	100	182	127	105	1,200	194	104	169	148	190	456	...
Reduction per cent.*	82·2	98·7	99·2	90·5	95·2	90·2	98·4	98·0	98·2	93·1	99·3	95·9	95·3
DEEP WELLS (Kent Company).													
Bath Well	9	19	80	26	27	12	14	5	5	7	8	6	...
Garden Well	48	20	4	4	—	24	18	—	8	..	5	12	...
New Well	12	10	5	12	20	14	8	59	27	30	65	67	...
Supply	82	75	140	163	50	26	44	116	115	857	40	68	...

* These reductions apply only to the East London supply.

Summary of the Monthly Reports on the Bacteriological Condition of the London Water-Supply presented to the Local Government Board by Percy F. Frankland in 1888.

Number of Colonies obtained from 1 c.c. of Water by Gelatine-plate Cultivation.

Name of supply.	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	Average for year.
THAMES.													
Thames water, unfiltered (Hampton)	92,000	40,000	66,000	13,000	1,900	3,500	1,070	3,000	1,740	1,130	11,700	10,600	..
Chelsea	127	152	54	38	43	63	87	82	86	14	82	71	..
West Middlesex	60	146	408	158	71	56	27	11	26	83	31	16	..
Southwark	177	766	742	47	47	24	85	27	106	85	167	136	..
Grand Junction	90	349	617	56	77	40	15	4	20	16	25	208	..
Lambeth	189	820	321	157	64	140	55	33	92	27	126	151	..
Reduction per cent.	99·9	98·9	99·4	99·8	96·8	98·1	96·8	99·3	96·8	97·8	99·3	98·9	98·4
LEE.													
Lee water, unfiltered (Chingford)	31,000	26,000	63,000	84,000	1,124	7,000	2,190	2,000	1,670	2,310	57,500	4,400	..
New River	27	90	169	77	37	60	11	13	—	15	70	91	..
East London	2,038	780	859	193	209	266	253	57	64	63	49	141	..
Reduction per cent.*	93·4	97·0	99·4	99·8	81·4	96·2	88·4	97·2	96·2	97·3	99·9	96·8	95·3
DEEP WELLS (Kent Company).													
Bath Well	6	47	6	33	7	17	8	—	8	4	84	—	..
Garden Well	5	19	8	4	27	71	5	—	10	9	18	—	..
New Well	12	4	5	7	8	20	4	3	—	96	19	—	..
Supply	55	81	15	69	139	219	32	42	52	55	54	63	..

* These reductions apply only to the East London supply.

Ward, on March 8, 1892, at a point more than a mile above Staines, and sufficiently distant from the Windsor district to render it very improbable that any direct contamination thence need be feared; as a matter of fact, the analyses show that this water was by no means rich either in bacteria or organic matter, and, for an open river, was remarkably pure (see p. 182).

As this sample was not received by me until some days after its collection, the number of micro-organisms which I found in it does not afford any insight into the bacterial condition of the river at the time, but on this subject I have already collected a large amount of information in the course of the regular monthly examinations of the London water supply which I made for the Local Government Board during the three years 1886, 1887, and 1888. The results of these examinations, which are recorded in the preceding three tables, clearly indicate (1) the seasonal variations which the number of bacteria in the unfiltered waters of the Rivers Thames and Lee undergoes; (2) the great reduction in these numbers which is effected by the storage and sand-filtration to which these waters are subjected at the waterworks before distribution; (3) the very small number of bacteria present in the deep-well water of the Kent Company.

Two entirely independent series of experiments have been made with this sample, the difference between the two series being in the number of anthrax organisms which were introduced into the water. Thus in the First Series, a comparatively small number of anthrax bacilli were put into the water, whilst in the Second Series the number introduced was very much larger. There was this further difference between the two series of experiments that the *Bacillus anthracis* employed had a totally different origin in the two cases. The use of the organism from two distinct sources in the way indicated is in my opinion of great importance as eliminating the possibility of any special and exceptional characters having become impressed on the particular cultivation employed.

Experiments with Thames Water (First Series).

We will direct our attention in the first instance to what we have called the " First Series " of experiments, in which the sporiferous *Bacillus anthracis* was introduced in comparatively small numbers only into the water.

This First Series of experiments includes four sub-series, in each of which the Thames water was employed in a different condition. Thus—

(1.) Experiments made with the Thames water in its natural state.

(2.) Experiments made with the Thames water after removing the coarser suspended particles by filtration through Swedish filter

paper. This was done, as it was quite conceivable that the presence of comparatively large suspended particles should exercise a marked influence on the behaviour of the anthrax introduced, whilst this filtration would not remove more than a portion of the bacteria already present in the water in its natural state. It was, moreover, especially desirable to ascertain whether the removal of these coarser suspended particles would influence the result, as in the following sub-series (3) all suspended particles, including the water bacteria themselves, were removed prior to the introduction of anthrax.

(3.) Experiments with the Thames water after removing all suspended particles, including bacteria, by filtration through porous porcelain (Chamberland filter), or, in other words, Thames water sterilised without the agency of heat.

(4.) Experiments with the Thames water after filtration through Swedish paper as in sub-series (2), and subsequent sterilisation with steam.

Chemical Composition of the Thames Water employed.—The water was submitted to analysis, (a) *in its natural condition*, (b) *after filtration through Swedish paper*, and (c) *after filtration through porous porcelain*, with the following results :—

Results of Analysis expressed in Parts per 100,000.

	(a).	(b).	(c.)
Total solid residue (dried at 100° C.)	35·20	33·60	33·60
Organic carbon.	0·207	0·212	0·189
„ nitrogen	0·023	0·039	0·021
Ammonia (free)	0·004	0·003	0·007
„ (albuminoïd)	0·016	0·010	0·014
Oxygen consumed by organic matter, as measured by the reduction of permanganate acting for three hours in the cold	0·076	0·054	0·064
Nitrogen as nitrates and nitrites	0·230	0·272	0·229
Total combined nitrogen	0·256	0·313	0·256
Chlorine	1·6	1·65	1·7
Temporary hardness	17·3	17·1	16·5
Permanent „	5·1	5·3	5·9
Total „	22·4	22·4	22·4
Remarks	very turbid	clear	clear

These results show that the sample contained only a moderate amount of organic matter, and was representative of the river when in its purest condition. They also show that neither the filtration

through paper nor through porcelain exerts any material effect on the chemical composition of the water, such differences as appear being almost within the limits of experimental error, especially when it is borne in mind that (a), (b), and (c) were taken from different bottles.

Filtration of the Water through Porcelain.—The sterilisation of the water without application of heat was conveniently effected in the following manner :—

A glass cylinder (A) open at both ends is tightly fitted to an india-rubber stopper (CC), which is also fitted into the strong

Fig. 1.
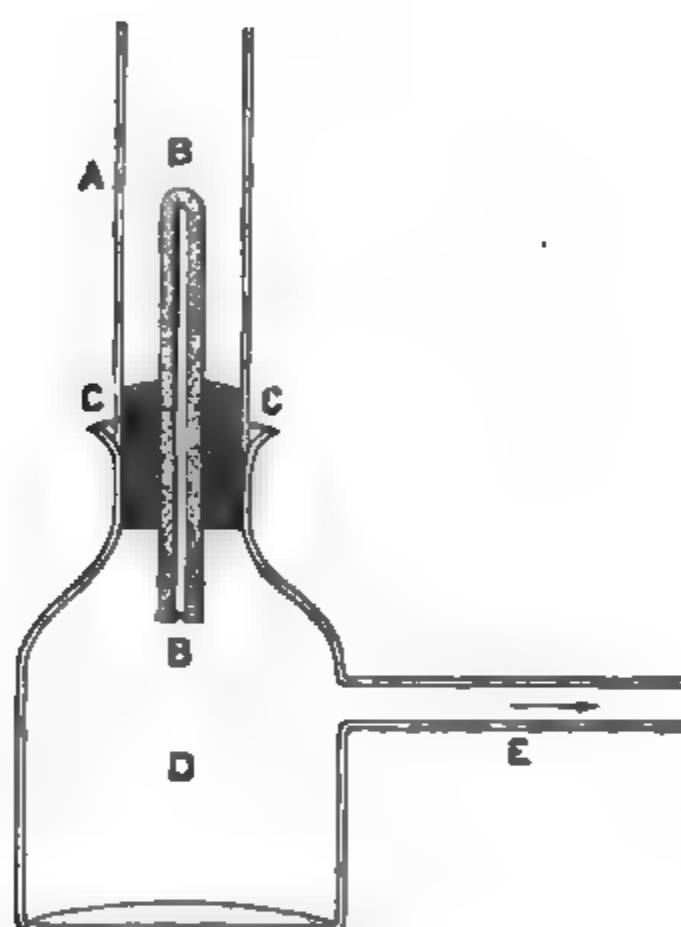
Porcelain Filter.

glass bottle (D), a porous cylinder of biscuit-porcelain of the construction shown in section in (BB) also passes through the same india-rubber stopper. The bottle (D) has a lateral tubulure (E) which is plugged with sterile cotton-wool. The whole of this apparatus is sterilised in position by placing it bodily in the steam-steriliser for several hours on three successive days, after which it is ready for use. The water to be filtered is poured into the glass cylinder (A), and the tubulure (E) is connected by means of pressure tubing with a water-pump. On thus reducing the pressure inside (D), the water in (A) is forced by atmospheric pressure through the porous cylinder (BB). In order to remove the filtered water

o 2

from (D), the cotton-wool plug is extracted from (E), the latter tube is carefully heated with a Bunsen flame to destroy any organisms that may be resting on its open extremity, and by inclining (D) the water can be made to flow out into the sterilised vessels placed for its reception without undergoing any contamination.

Infection of the Water with Anthrax.—In this first series the anthrax bacilli were taken from an agar-agar cultivation of about three weeks age, in which it was known by previous microscopic examination that spores were abundantly present.

The surface of the agar-agar was scraped with a sterile platinum loop, care being taken to remove the growth with as little as possible of the culture material. Five loops full in all were taken and transferred to a small sterile stoppered bottle containing about 50 c.c. of Thames water which had been steam-sterilised. The contents were then violently agitated for some fifteen minutes in order to break up the conglomerations of bacilli and spores, and effect as uniform a distribution as possible. This may be termed the " first attenuation."

From this first attenuation four portions of 2 c.c. each were removed with sterile pipettes and introduced respectively into four large flasks, each containing about 2 litres of the waters for experiment, viz. :—

(a.) Thames water, in natural state.
(b.) Ditto after filtration through Swedish paper.
(c.) Ditto after filtration through porcelain.
(d.) Ditto, after sterilisation by steam.

The waters thus infected were well shaken in the large flasks containing them so as to ensure complete mixture, and the contents of each flask was then distributed in a number of small sterilised conical flasks plugged with sterile cotton wool. In the case of each of these infected waters some of the small conical flasks were placed in an incubator maintained at 18—20° C., the summer temperature of surface waters, whilst others were put into a refrigerator in which a temperature of 6—10° C. was preserved.

The distribution and arrangement will be readily apparent from the following tabular statement :—

Thames Water in Natural State.

| Un-infected | { 2 flasks in incubator. / „ refrigerator. | Infected | { 4 flasks in incubator. / 3 „ refrigerator. |

Thames Water after Filtration through Swedish Paper.

| Un-infected | { 2 flasks in incubator. / „ refrigerator. | Infected | { 5 flasks in incubator. / „ refrigerator. |

Thames Water after Filtration through Porcelain.

Un-infected $\left\{\begin{array}{l}\text{2 flasks in incubator.}\\ \text{ ,, \quad refrigerator.}\end{array}\right.$ $\Big|$ Infected $\left\{\begin{array}{l}\text{5 flasks in incubator.}\\ \text{ ,, \quad refrigerator.}\end{array}\right.$

Thames Water after Sterilisation by Steam.

Un-infected $\left\{\begin{array}{l}\text{2 flasks in incubator.}\\ \text{ ,, \quad refrigerator.}\end{array}\right.$ $\Big|$ Infected $\left\{\begin{array}{l}\text{5 flasks in incubator.}\\ \text{ ,, \quad refrigerator.}\end{array}\right.$

Thus, there were in all 16 uninfected and 37 infected flasks employed.

Note.—Throughout the subsequent account of the series, all flasks placed in the incubator are designated thus: "1 I," "2 I," "3 I," &c., whilst flasks placed in the refrigerator are distinguished as "1 R," "2 R," "3 R," &c.

Examination of the Waters for the Presence of Anthrax.—The bacteriological examination for the presence of anthrax was in general made by the ordinary process of gelatine plate cultivation. This method of identifying the presence of anthrax is attended with but little difficulty if no other organisms are simultaneously present, as in the case of the waters sterilised by steam, and by filtration through porcelain. The anthrax colonies develop with such facility in the gelatine medium, and are of such a characteristic appearance even to the naked eye, but especially when seen through a low power of the microscope, that no doubt can be entertained as to their identity. On the other hand, the very greatest difficulty attends, as will be seen, the recognition of anthrax in the presence of the ordinary water bacteria, partly because the colonies of some of the latter grow much more quickly than those of anthrax, but especially because many of these water bacteria cause such rapid liquefaction of the gelatine that the greater part, or even the whole, of the film may be destroyed before the anthrax colonies have had time to become visible. In order to overcome this difficulty, I have devised a method of destroying nearly the whole of these liquefying bacteria without injuring more than a part of the anthrax spores, and thus rendering possible the development and recognition of the colonies from the latter, even when they are present in water along with vast multitudes of the ordinary water bacteria. The nature of this special method will be described later. The gelatine plates were invariably incubated at a temperature of 18—20° C., and in order to give every opportunity for the anthrax colonies to make their appearance, the incubation was carried on as long as possible. On this account the numerical estimation of the other colonies was made a subsidiary matter, and in consequence of the extensive liquefaction which had often taken place, the accuracy of the numbers found has often been interfered with; in fact, discrepancies in the number of colonies found on dupli-

Tabular Description of Bacteria isolated from the Waters of the Rivers Thames and Lee, and from Deep Wells in the Chalk (Grace and Percy Frankland, 'Zeitschrift für Hygiene,' vol. 6, 1889).

Name of micro-organisms and where found.	Microscopic appearance.	Appearance in gelatine-plate culture.	Gelatine-tube cultivations.	Agar-agar cultivations.	Broth cultivations.	Potato cultivations.	Growth and action in nitrate solution.
No. 1.—*Bacillus arborescens.* Filtered river water of Thames and Lee.	Slender bacillus with rounded ends, about 2·5 μ long and 0·5 μ broad. Hangs together in twos and threes, but in broth cultures forms long wavy threads. No spore formation observed. Is capable of vibratory movement only.	Under a low power (×100 diameters) is seen to form a thin axial stem, from both ends of which root-like branches extend, which gradually assume the appearance of a wheat-sheaf. Slow liquefaction of the gelatine takes place, and near the colony the surface of the gelatine exhibits beautiful iridescent colours.	Slowly liquefies the gelatine, producing a yellow deposit.	Produces a dirty orange coloured pigment, and grows slowly.	Renders the liquid turbid, and produces yellow deposit. No pellicle forms on the surface.	Produces a luxuriant deep orange growth.	No visible growth takes place; neither is any reduction of the nitrate to nitrite effected.
No. 2.—*Bacillus aquatilis.* Deep-well water obtained from the chalk.	Very similar in appearance to No. 1, and also forms wavy threads, sometimes as long as 17 μ and more. No spores observed. Vibratory movement only.	In the depth the colonies at first appear smooth rimmed, but the contour gradually becomes more and more irregular. On reaching the surface show liquefaction of the gelatine commences, and convoluted bands of threads extend from the centre to the periphery.	Grows extremely slowly; forms a slightly yellow expansion on the surface, but hardly any growth appears in the depth. Later slight liquefaction takes place.	Produces a small shining yellow growth.	Renders it turbid, and produces a whitish deposit. No pellicle is formed.	Hardly any growth at all.	Grows abundantly, but fails to convert the nitrate to nitrite.
No. 3.—*Bacillus liquidus.* Rivers Thames and Lee.	Short thick bacillus with rounded ends; occurs usually in pairs, the dimensions of which are very variable (from 1·5 to 3·5 μ). No spores were found. It is very motile.	In the depths the colonies are smooth-rimmed; later they become jagged. It grows rapidly and causes extensive liquefaction of the gelatine, producing large circular depression with almost clear contents.	Grows rapidly, forming a funnel-shaped depression, which is filled with turbid liquid. A thin pellicle forms later on the surface.	Produces a small shining expansion, and grows luxuriantly.	Renders it turbid, producing an abundant deposit, also a pellicle on the surface.	Produces a flesh coloured pigment.	Powerfully reduces the nitrate to nitrite.

No. 4.—*Bacillus vermicularis.* River Lee.	Large bacillus with rounded ends, in length about 2 to 3 μ, and about 1 μ broad. Forms extensive vermiform threads. Produces oval spores about 1·5 μ long, and 1 μ broad. It is not motile.	The colonies in the depth are irregular in contour. This irregularity increases as the liquefaction commences, and the colony approaches the surface. The periphery is seen to consist of closely packed wavy bands of bacilli, whilst the centre of the colony looks irregular and wrinkled.	Forms a moist shining grey expansion, whilst in the depth the path of the needle is indicated by a slight sword-like growth. Slow liquefaction of the gelatine takes place.	Produces a smooth shining grayish pigment.	The liquid remains clear, whilst a considerable white flocculent deposit is formed.	Produces a thick irregular flesh coloured pigment.	Powerfully reduces the nitrate to nitrite.
No. 5.—*Bacillus nubilus.* River Thames.	Slender bacillus, about 3 μ long, and 0·3 μ broad. Forms long wavy threads in broth cultures. No spores were found. The isolated bacilli exhibit violent rotatory movements, but the threads are quite stationary.	Forms cloudy undefined patches, which under the microscope are seen to consist of a thick and tangled mass of bacillar threads. Rapid liquefaction of the gelatine takes place.	The surface is liquefied, but all along the path of the needle a series of horizontal circular plates arise, having a delicate cloud-like appearance. Later the whole of the gelatine becomes liquid.	Produces a thin opalescent blue-violet expansion, the edges of which exhibit later a distinct violet fluorescence.	Renders it turbid and produces a dirty white deposit, whilst the surface becomes covered with a thin pellicle.	Produces a delicate and slightly yellow growth which is barely visible.	Reduces a very small proportion of the nitrate to nitrite.
No. 6.—*Bacillus ramosus.* Frequently found in Rivers Thames and Lee, but never in deep-well water.	Much resembles *B. subtilis.* The individual bacilli are about 7 μ long and 1·7 μ broad, the ends being distinctly rounded. It gives rise to long threads, also spores. Is capable of only slight oscillatory movement.	The colonies are seen to consist of cloudy centres with tangled root-like branches extending in every direction. Later liquefaction of the gelatine takes place.	The whole contents of the tube become impregnated with fluffy ramifications. Later liquefaction takes place, and a tough pellicle forms on the surface.	Grows rapidly over the whole surface, whilst in the depth the characteristic "branching" is again visible.	Forms a light flocculent deposit, and produces later a tough and wrinkled pellicle on the surface.	Produces a dry and uniform expansion, which is almost quite white.	Powerfully reduces the nitrate to nitrite.
No. 7.—*Bacillus aurantiacus.* Deep-well water.	Short fat bacillus of very variable dimensions. It grows in pairs, and also forms long threads. The short bacilli are about 1·7 μ long, and nearly half as wide as long. No spores were observed. The individual bacilli are motile.	Produces bright orange pin-heads. Under the microscope the depth colonies are seen to be smooth rimmed. No liquefaction of the gelatine takes place, and its growth is slow.	A shining orange coloured expansion forms on the surface, whilst hardly any growth is visible in the depth.	Forms a bright orange expansion, which does not extend much beyond the point of inoculation.	The liquid remains clear, whilst a slightly orange coloured deposit is produced. A thin pellicle forms on the surface, which exhibits here and there bright spots of orange colour.	Produces a thick and magnificently brilliant red-orange pigment which is however restricted to the point of inoculation.	Reduces the nitrate to nitrite only very slightly.

Tabular Description of Bacteria isolated from the Waters of the Rivers Thames and Lee, and from Deep Wells in the Chalk (Grace and Percy Frankland)—*continued*.

Name of micro-organisms and where found.	Microscopic appearance.	Appearance in gelatine-plate culture.	Gelatine-tube cultivations.	Agar agar cultivations.	Broth cultivations.	Potato cultivations.	Growth and action in nitrate solution.
No. 8.—*Bacillus viscosus.* Found very frequently in unfiltered river water of the Thames and Lee, also occasionally in the same water after filtration, whilst it is only rarely found in deep-well water.	Short bacillus with rounded ends, from 1·5 to 2 μ long, about three or four times as long as broad. Occurs usually in pairs. No spores observed. It is exceedingly motile.	In the depth the colonies appear smooth-rimmed; later, when liquefaction commences, the periphery exhibits fine hair-like extensions. The gelatine is rapidly liquefied, and each colony is surrounded by a green fluorescent zone.	Causes rapid liquefaction of the gelatine, producing green fluorescence throughout the contents of the tube, which becomes excessively viscid.	The whole surface rapidly assumes a green appearance, and a smooth green-white expansion is produced.	Renders the liquid very turbid and viscid. Later a thin green-white pellicle is formed.	Produces a moist and shining chocolate coloured expansion, which extends over the whole surface.	No reduction of the nitrate takes place.
No. 9.—*Bacillus violaceus.* Originally found in River Spree water, but found by us also in the Rivers Thames and Lee, also in deep-well water from the chalk.	Short bacillus, varying in size, about 1·7 μ in length and 0·8 μ in width. It generally occurs in pairs. When grown on agar it assumes a far more slender appearance, and also gives rise to short threads. Spore formation was observed. It is motile, but restricted chiefly to vibratory and rotatory movements.	In the depth the colonies appear irregular in contour, which increases with the age of the colony. It forms a circular depression in the gelatine, and later the characteristic violet pigment makes its appearance. Liquefaction does not take place rapidly.	Liquefaction takes place in the form of a funnel, the liquid becomes turbid and at the bottom of the funnel the violet pigment collects.	Forms a beautiful deep violet coloured expansion, which spreads over the whole surface.	The liquid is rendered slightly turbid, and later on a violet deposit is produced.	It is unable to grow on potatoes.	Powerfully reduces nitrates to nitrites.

cate plates cannot fail to take place if they are preserved until widespreading liquefaction of the gelatine has occurred.

1. *Bacteriological Examination of the Thames Water previous to Infection with Anthrax.*

From Table I it will be seen that the unfiltered water contained a large number of micro-organisms, which was, however, much reduced by the simple process of filtration through Swedish paper. The colonies obtained showed the micro-organisms to be of numerous different kinds, many of them being easily recognisable as belonging to the forms which have already been described and figured by me three years ago (Grace C. and Percy F. Frankland, 'Zeitsch. f. Hygiene,' vol. 6, 1889) as occurring in Thames water, and a brief *résumé* of which is given in the following table. Owing to a large number of the colonies causing liquefaction of the gelatine, their numerical estimation is much interfered with, and in order to some extent obviate this difficulty, it will be seen that plates were not only poured with the undiluted waters, but also with the waters after large dilution (50 times their volumes) with sterilised water, so as to obtain a smaller number of colonies on the plate. This expedient has always been resorted to in cases where an inconveniently large number of colonies was to be expected; in all cases, however, the results are calculated to the number of colonies obtained from 1 c.c. of the water in the undiluted state.

Table I also shows that the filtration through porcelain, as well as the steaming, were effectual in sterilising the water, the number of colonies obtained in these cases being no greater than would appear on blank plates.

The unfiltered Thames water, and that which had been passed through Swedish paper, were again examined on October 26, 1892, or more than seven months after the experiments were commenced; in both cases the flasks which had been kept in the incubator contained more organisms than those which had been in the refrigerator, but in all cases the numbers were comparatively small, and in the unfiltered water had fallen much below what they were at the beginning.

Table I.—Uninfected Thames Water.   First Series of Experiments, commenced March 18, 1892.

Dates on which plates were poured.	Water used.	Number of plate.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies from 1 c.c. of water. Total number.
18.3.92	Unfiltered	17	5	c.c. $\frac{1}{4}$	15,876
		18	5	$\frac{1}{8}$	17,163
		19	7	$\frac{1}{20}$	38,700
		20	7	$\frac{1}{100}$	20,400
18.3.92	Paper filtered	13	4	$\frac{1}{4}$	2,952
		14	7	$\frac{1}{4}$	8,256
		15	7	$\frac{1}{20}$	2,600
		16	7	$\frac{1}{100}$	1,100
18.3.92	Porcelain filtered	21	8	1	2
		22	8	$\frac{1}{4}$	6
18.3.92	Steamed	23	8	1	5
		24	8	$\frac{1}{4}$	0
26.10.92	Unfiltered. 1 I*	510	8	$\frac{1}{4}$	4,312
	1 I	511	8	$\frac{1}{4}$	4,851
	1 I	514	4	$\frac{1}{8}$	4,950
	1 I	515	4	$\frac{1}{10}$	6,100
	1 R	512	8	$\frac{1}{4}$	1,623
	1 R	513	8	$\frac{1}{4}$	1,868
	1 R	515	4	$\frac{1}{20}$	1,950
	1 R	517	4	$\frac{1}{100}$	1,900

26.10.92	Paper filtered.				
	1 I	518	8	4/...	1,986
	1 I	519	8		3,150
	1 I	522	3		4,400
	1 I	523	8		4,800
	1 R	520	9		935
	1 R	521	8		1,090
	1 R	524	8		1,600
	1 B	525	8		1,450

* For explanation of the system of naming the flasks containing the experimental waters, see Note, p. 185.

2. *Bacteriological Examination of the Unsterilised Unfiltered Thames Water (First Series) after Infection with Anthrax.*

Table II brings out a number of points. In the first place the extreme difficulty of discovering, by ordinary plate cultivation, a particular micro-organism when present in only small numbers alongside of vast multitudes of other forms, is particularly noteworthy. Special reference was made to this difficulty in our First Report, and it is most strikingly brought out in these experiments. From the experiments made with the steamed and porcelain-filtered Thames water, to be described below, we know that the number of anthrax organisms in this unfiltered Thames water must have amounted to at least 30—40 per cubic centimetre, yet only in one out of the numerous plate cultivations made with this water was anthrax discovered, and then only a single colony was found on a plate poured on the day that the infection with anthrax was made.

The principal obstacles to such discovery are two : firstly, in consequence of the extremely large number of micro-organisms present in the water, it is only possible to take a very small volume (not more than, say, $\frac{1}{10}$ c.c.) for cultivation, in which, therefore, the chance of anthrax organisms (if introduced in such small numbers as in the present series of experiments) being present is very remote; whilst, secondly, owing to the rapid liquefaction of the gelatine caused by many of these water bacteria, it is not possible generally to incubate the plates for a sufficient length of time to admit of the proper development of the anthrax colonies; this is more particularly the case when, as here, the anthrax is present in the form of spores, which take time to germinate.

In order to obviate these difficulties attending the discovery of anthrax in the presence of large numbers of water bacteria causing liquefaction, I have tried a number of special devices, of which I need, however, only describe the one which proved the most useful.

Previous experience had shown me that a large proportion of the organisms present in water, and more especially those causing liquefaction of the gelatine, are very sensitive to a temperature even considerably below that of boiling water, whilst the spores of anthrax in their normal state will withstand such temperatures for a considerable length of time.

In order to turn these properties to practical account, portions (1 c.c. or 3 c.c.) of the anthrax-infected Thames water under consideration were mixed with a little sterile broth (1 c.c.), and heated for periods of two or five minutes to 50° C., to 70° C., and to 90° C., after which treatment they were submitted to ordinary plate cultivation. The first of these experiments was made on March 31, *1892, and is recorded* in the above Table II. The infected Thames

water at this time contained upwards of 100,000 water bacteria in 1 c.c., yet after heating as above for five minutes to 50° C., only from 35 to 39 colonies per cubic centimetre made their appearance, and amongst these several were easily recognisable as those of anthrax. Again, on the same day, other portions of the same water were heated to 70° C. for two minutes, after which only from 10 to 30 colonies per cubic centimetre made their appearance, amongst which from 4 to 10 were recognisable as anthrax. Other portions of the same water were heated on the same day to 90° C. for two minutes, with the result that only from 7 to 10 colonies per cubic centimetre appeared, of which from 3 to 6 were found to be anthrax.

Thus by this simple method comparatively large volumes (up to 3 c.c. have been used, but there is no reason why even larger quantities should not, if necessary, be employed) of water swarming with water bacteria can be operated on and sifted, so to speak, for anthrax.

From Table II it will be seen that this method was repeatedly employed on the anthrax-infected unfiltered Thames water in question, in most cases the temperature of 70° C. for two minutes being resorted to.

Employing this method it will be seen from the table that *it became more and more difficult to discover anthrax in the water, although even after nearly four months anthrax could still be just traced both in the water, which had remained at summer* (18—20° C.) *temperature in the incubator, as well as in that preserved at the winter temperature* (6—10° C.) *of the refrigerator.*

It now became of interest to ascertain whether the water in which anthrax could just be barely traced by cultivation contained that anthrax in a virulent state. In consequence of the delay which occurred in my obtaining the necessary licence to perform these experiments, I was not able to attack this problem until October 7, 1892, or nearly seven months after the water was infected with anthrax.

Animal Experiment No. 1.—On October 7, 1892, 1 c.c. of water from " Flask 3 I, unfiltered Thames water infected with anthrax, March 18, 1892," was subcutaneously injected into a white mouse. The mouse did not succumb to anthrax, but is still living, 32 days after the operation.

Animal Experiment No. 2.—On the same day, October 7, 1892, 1 c.c. of water from " Flask 3 R, unfiltered Thames water, infected with anthrax, March 18, 1892," was subcutaneously injected into a white mouse. This mouse lived for 18 days 20$\frac{1}{2}$ hours after the operation, and, of course, did not succumb to anthrax ; no bacilli could be found in the spleen, nor by cultivation in gelatine.

From these experiments it is obvious that on October 7, 1892, or nearly seven *months* after infecting this unfiltered Thames water

with sporiferous anthrax bacilli, the latter had been so much reduced in number, that a whole cubic centimetre of the water, irrespectively of whether it had been kept at the winter temperature of the refrigerator, or at the summer temperature of the incubator, was unable to cause the death of a mouse, the most sensitive of animals to anthrax.

It was in the next instance necessary to ascertain whether the sporiferous anthrax had actually perished outright in this water, or whether it was still present in a living state only in very small numbers. To determine this point, the following plan was adopted:— The two flasks referred to above, 3 I and 3 R, were each treated with 5 c. c. of sterile broth on October 15, 1892, and placed in an incubator at 37° C., so as to encourage the growth and multiplication of any anthrax bacilli or spores that might still be living in them. With these nourished waters, so to speak, then, the following two experiments were made:—

Animal Experiment No. 16.—On October 18, 1892, 0·4 c.c. from the Flask 3 I, to which broth had been added on October 15, 1892, was subcutaneously injected into a white mouse. The mouse died within 1 day 19 hours, and, although no bacilli could be microscopically detected in the spleen, a gelatine tube inoculated from the spleen developed a typical growth, and in this the characteristic anthrax bacilli were subsequently found, thus leaving no doubt that the mouse had died of anthrax.

Animal Experiment No. 24.—On October 29, 1892, 0·5 c.c. from the Flask 3 R, to which broth had been added on October 15, 1892, was subcutaneously injected into a white mouse. The mouse died within 2 days 18 hours ; the spleen was found to be full of anthrax bacilli, and gelatine cultivations prepared from the spleen yielded the characteristic growths, thus leaving no doubt that the animal succumbed to anthrax.

These two experiments clearly demonstrate that the sporiferous anthrax bacilli had not become actually extinct in this unfiltered Thames water (either at winter or summer temperature), but had only undergone great numerical decline, for on adding broth to the waters these straggling forms multiplied sufficiently to cause the death of the mice which received subcutaneous injections of them.

Of subsidiary interest in Table II is the insight furnished into the behaviour of the ordinary water bacteria at the temperatures in question. These numbered about 10,000 per cubic centimetre at the outset,* whilst after 4 days (March 22, 1892) about

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* This does not refer to the number present in the water at the time of its collection from the Thames, but at the time of its infection with anthrax several days later. The sample was collected on March 8, and the infection was made on the *18th of the same month.*

Table II.—Anthrax in Thames Water. First Series of Experiments. Unfiltered Thames Water, Infected with Anthrax, March 18, 1892.

Dates on which plates were poured.	Number of plate.	Particular flask employed. *	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies from 1 c.c. of water.		Remarks.
					Total number.	Number of anthrax.	
18.3.92	9	*	4	c.c. $\frac{1}{10}$	8,540	0	The Thames water collected on 8.3.92, remained in stoppered bottles at 10° C. until 18.3.92, when these experiments were commenced.
	10		7	$\frac{1}{10}$	Too much liquefied	10	
	11		7	$\frac{1}{100}$	18,600	0	
	12		7	$\frac{1}{1000}$	13,200	0	
22.3.92	34	8 I	8	$\frac{1}{10}$	Too much liquefied	0	
	35	8 I	8	$\frac{1}{10}$	49,800	0	
	42	8 I	4	$\frac{1}{100}$	113,850	0	
	43	8 I	4	$\frac{1}{1000}$	91,850	0	
	86	8 B	8	$\frac{1}{10}$	Too much liquefied	0	
	87	8 B	8	$\frac{1}{10}$	127,660	0	
	44	8 B	4	$\frac{1}{100}$	143,550	0	
	45	8 B	4	$\frac{1}{1000}$	141,900	0	
31.3.92	79	2 B	4	3	35	Certainly a few	3 c.c. water + 1 c.c. broth, heated for 5 min. at 50° C.
	80	2 B	4	1	89	8	1 c.c. ,, ,, 2 min. at 70° C.
	81	2 R	4	8	10	4	8 c.c. ,, ,, ,,
	82	2 R	4	1	80	10	1 c.c. ,, ,, ,,
	83	2 B	4	8	7	8 or 4	3 c.c. ,, ,, 2 min. at 90° C.
	84	2 B	4	1	10	6	1 c.c. ,, ,, ,,

* For explanation of the system of naming the flasks containing the experimental waters, see Note, p. 185.

Table II—*continued*.

Dates on which plates were poured.	Number of plate.	Particular flask employed.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies from 1 c.c. of water.		Remarks.
					Total number.	Number of anthrax.	
				c.c.			
6.4.92	86	2 I	6	8	56	2	3 c.c. water + 1 c.c. broth, heated for 2 min. at 70° C.
	87	2 I	6	1	80	6 or 7	1 c.c. ,, ,, ,,
	88	2 I	6	8	19	0	3 c.c. ,, 2 min. at 90° C.
	89	2 I	6	1	23	2	1 c.c. ,, ,, ,,
3.5.92	167	2 I	3	1	Too much liquefied	0	
	168	2 I	3	⅓	21,210	0	
	171	2 I	4	⅟₁₅	9,500	0	
	172	2 I	4	⅟₁₅₀	9,500	0	
10.5.92	180	2 I	5	8	18	8	3 c.c. water + 1 c.c. broth, heated for 2 min. at 70° C.
	181	2 I	5	2	29	9	2 c.c. ,, ,,
24.6.92	290	2 I	3	2	5,778	0	3 c.c. water + 1 c.c. broth, heated for 2 min. at 70° C.
	291	2 I	3	⅟₄	6,454	0	2 c.c. ,, ,,
	294	2 I	4	⅟₁₅	6,200	0	
	295	2 I	5	⅟₁₅	9,000	0	
	298	2 I	4	3	Too much liquefied	0	
	299	2 I	6	2	16	0	
	302	3 R	3	2	1,650	0	
	303	3 R	3	⅟₁₅	2,290	0	
	306	3 R	4	⅟₁₅	2,700	0	
	307	3 R	5	⅟₁₅₀	3,250	0	
	310	3 R	5	3	9	0·3	
	311	3 R	6	2	15	0	
9.7.92	349	8 I	5	8	10	0·3	3 c.c. water + 1 c.c. broth, heated for 2 min. at 70° C.
	350	8 I	5	2	18	1	2 c.c. ,, ,,
	351	3 R	4	8	11	0	3 c.c. ,, ,,
	352	3 R	5	2	16	1·5	2 c.c. ,, ,,

80,000 were found in the incubated, and upwards of 100,000 in the refrigerated, water. After 46 days (May 3, 1892) they numbered about 13,000 in the incubated water, whilst after 98 days (June 24, 1892) the numbers in the incubated water were about 7,000, and from 2.000 to 3,000 in the refrigerated. These latter figures are much the same as were obtained in the uninfected waters (see Table I) after upwards of 7 months.

3. *Bacteriological Examination of the Unsterilised Thames Water (First Series), Filtered through Swedish Paper and Infected with Anthrax.*

The experiments with the Thames water which had been filtered through Swedish paper prior to infection with anthrax, and which are recorded in Table III (pp. 198 and 199), yielded very much the same results as those made with the unfiltered Thames water recorded in Table II. We find a similar multiplication followed by diminution in the number of water bacteria, both in the flasks kept at summer and winter temperatures respectively. In no single instance was anthrax detected by ordinary plate cultivation, but in the refrigerated water it was found by the special method after 98 days (June 24, 1892), whilst with the incubated water the same method failed to find anthrax after 53 days (May 10, 1892), and again after 98 days (June 24, 1892), whilst in two final examinations, after 113 days (July 9, 1892), one again gave a negative result, whilst the other yielded a feeble growth, presenting a very doubtful resemblance to anthrax. It may be taken, therefore, that in the water preserved at summer temperature the degeneration of anthrax was markedly more rapid than in that kept at the winter temperature.

This infected paper-filtered Thames water was also examined for virulence by direct experiment, as follows :—

Animal Experiment No. 8.—On October 15, 1892, 1 c.c. of water from the flask " 1 R, paper-filtered Thames water, infected with anthrax, March 18, 1892," was subcutaneously injected into a white mouse. The mouse died within 4 days 17 hours ; there was very extensive œdema, and the spleen was not much enlarged ; no bacilli were microscopically found in the spleen, but the characteristic growth was obtained on cultivation in gelatine, thus leaving no doubt that the animal had succumbed to anthrax, although in an attenuated form.

The result of this experiment is interesting in several respects. Thus, firstly, it shows that in this water *anthrax was still present, after 7 months, and in sufficient numbers in 1 c.c. to cause the death of the mouse, whilst it will be remembered that in the unfiltered Thames water this was not the case, so that apparently the removal of a certain propor-tion of the water bacteria by paper filtration had been conducive to the preservation of the anthrax.*

Table III.—Anthrax in Thames Water. First Series of Experiments. Paper Filtered Thames Water, Infected with Anthrax, March 18, 1892.

Date when plates were poured.	Particular flask employed.	Number of plate.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies from 1 c.c. of water.		
					Total number.	Number of anthrax colonies.	
22.3.92	1 I	38	3	c.c. $\frac{3}{5}$	Too much liquefied	0	
	1 I	39	3	$\frac{1}{5}$	"	0	
	1 R	40	3	$\frac{1}{10}$	"	0	
	1 R	41	3	$\frac{1}{10}$	"	0	
	1 I	46	3	$\frac{1}{25}$	"	0	
	1 I	47	3	$\frac{1}{125}$	385,000	0	
	1 R	48	3	$\frac{1}{25}$	Too much liquefied	0	
	1 R	49	3	$\frac{1}{250}$	677,000	0	
3.5.92	2 I	169	3	1	Too much liquefied	0	
	2 I	170	3	$\frac{3}{10}$	3,330	0	
	2 I	170	4	$\frac{1}{25}$	2,250	0	
	2 I	173	4	$\frac{1}{125}$	3,750	0	
10.5.92	2 I	182	5	3	2	0	3 c.c. water + 1 c.c. broth, heated for 2 min. at 70°C.
	2 I	183	5	2	4	0	2 c.c. "
24.5.92	1 R	215	3	$1\frac{9}{10}$	Too much liquefied	0	"
	1 R	216	3	$\frac{1}{25}$	"	0	"
	1 R	219	6	$\frac{1}{50}$	"	0	"
	1 R	220	6	$\frac{1}{125}$	"	0	

Date		Flask No.					
21.6.92	2 I	292	3	2	Too much liquefied	0	The plates were so much liquefied that the estimated numbers are uncertain.
	2 I	293	3	4	11,160	0	
	2 I	296	4	7.5	16,400	0	
	2 I	297	4	37.5	15,400	0	
24.6.92	2 I	300	4	3	Too much liquefied	0	3 c.c. water + 1 c.c. broth, heated for 2 min. at 70° C.
	2 I	301	4	2	11	0	2 c.c. ,, ,, ,,
	1 R	304	3	2	Too much liquefied	0	
	1 R	305	3	4	3,212	0	
	1 R	308	4	3.5	5,400	0	
	1 R	309	4	37.5	4,700	0	
	1 R	312	7	3	5	3	3 c.c. ,, ,, ,,
	1 R	313	7	2	6	3	2 c.c. ,, ,, ,,
9.7.92	2 I	353	4	3	2	0	3 c.c. water + 1 c.c. broth, heated for 2 min. at 70° C.
	2 I	354	10	2	17	1	2 c.c. ,, ,, ,,

* For explanation of the system of naming the flasks containing the experimental waters, see Note, p. 185.

That the anthrax had undergone considerable attenuation through its long residence in this unsterile water appears from its tardy causation of death with the non-typical symptom of but slight enlargement of the spleen.

4. *Bacteriological Examination of the Infected Thames Water Sterilised by Filtration through Porous Porcelain and by Steam respectively (First Series).*

The results recorded in Tables IV and V may be conveniently considered together. These tables exhibit the effect of introducing a small number of spore-bearing anthrax bacilli into Thames water previously sterilised, on the one hand by filtration through porous porcelain, and on the other by steam.

In these waters the recognition and numeration of the anthrax colonies in the plate cultivations is, of course, attended with no difficulty, and, indeed, it is only by means of these that an estimate of the number of anthrax organisms introduced into the unsterilised water can be formed. It will be seen that the number of anthrax germs introduced into each cubic centimetre of the waters of this Series I amounted to from 30 to 100. The fate of these in the unsterilised water we have already traced; in these sterilised waters it will be seen that they undergo little or no change in numbers during a period of upwards of 3 months, nor is there any material difference in their deportment in the waters sterilised in the two different ways.

Although these waters, which had been submitted to steam and filtration through porcelain respectively, were sterile in the first instance, it was only to be expected that in repeatedly opening the flasks for the purpose of preparing the plate cultivations, some would become contaminated with air-carried bacteria, &c., the presence of such intruding forms in a few of the flasks will be found recorded in the tables, but they generally gave rise to no difficulty in connexion with the plate cultivations.

It will be seen that in the case of these sterilised waters there is evidence of a very slight increase in the number of anthrax colonies after the first day, the number subsequently falling to about the original.

At the foot of Table IV will be seen the result of applying the special method of anthrax identification, which had to be relied on exclusively in the case of the unsterilised waters. On comparing the number of anthrax colonies found by this method with that obtained by ordinary plate cultivation, it will be seen that the greater portion of the anthrax organisms are either actually destroyed in the process *of heating* to 70° C., for 2 minutes, or, at any rate, are so far enfeebled *that they do not* subsequently develop in the gelatine. Thus, whilst

Table IV.—Anthrax in Thames Water. First Series of Experiments. Porcelain Filtered Thames Water, Infected with Anthrax, March, 18, 1892.

Date on which plates were poured.	Particular flask employed. *	Number of plate.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies from 1 c.c. of water.		
					Total.	Anthrax.	
18.3.92	..	3	8	c.c. ¼	72	72	
	..	4	8	¼	63	57	
22.3.92	1 I	28	6	⅛	128	128	
	1 I	29	6	1/16	131	181	
	1 R	32	6	¼	102	102	
	1 R	33	6	1/16	86	86	
8.5.92	1 I	177	7	2	144	144	
	1 I	178	7	1/10	125	125	
24.5.92	1 B	211	6	1 1/10	Too much liquefied through 1 liquid 80,287	..	} The water in flask had become contaminated by foreign organisms.
	1 R	212	6	1/10		55	
16.6.92	1 I	259	5	1	Contaminated with a mould	..	} Flask contaminated with a mould.
	1 I	260	5	1	"		
	1 R	263	5	3	Contaminated with a micrococcus	..	
	1 R	264	5	1	"		

* For explanation of the system of naming the flasks containing the experimental waters, see Note, p. 185.

Table IV—*continued.*

Date on which plates were poured.	Particular flask employed. *	Number of plate.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies from 1 c.c. of water.		
					Total.	Anthrax.	
29.6.92	2 I	326	8	3	Too much liquefied	..	Contaminated.
	2 I	327	8	1	..	:	,,
	1 R	328	6	3	..	43	,,
	1 R	329	6	1	..	44	,,
9.7.92	2 I	356	7	3	133	1	8 c.c. water + 1 c.c. broth, heated for 2 min. at 70°C.
	2 I	357	4	2	11	0	2 c.c. ,,
	1 R	358	10	3	11	2	8 c.c. ,,
	1 R	359	10	2	15	5	9 c.c. ,,

* For explanation of the system of naming the flasks containing the experimental waters, see Note, p. 185.

Table V.—Anthrax in Thames Water. First Series of Experiments. Steam-sterilised Thames Water, Infected with Anthrax, March 18, 1892.

Date on which plates were poured.	Particular flask employed. *	Number of plate.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies from 1 c.c. of water.		
					Total.	Anthrax.	
18.3.92	..	1	8	c.c. ½	82	80	
	..	2	8	½	42	39	
22.3.92	1 I	26	6	½	Innumerable	112	Contaminated with a micrococcus.
	1 I	27	6	½	,,	Small No.	
	1 R	30	6	1⁄10	60	60	
	1 R	31	6	1⁄10	70	70	
8.5.92	1 I	175	7	2	Too much liquefied through contamination		
	1 I	176	7	½	,,		
24.5.92	1 R	209	8	½	86	36	
	1 R	210	8	1⁄5	25	20	
16.6.92	2 I	257	6	3	33	33	
	2 I	258	6	1	41	40	
	1 R	261	6	3	45	45	
	1 R	262	6	1	46	46	

* For explanation of the system of naming the flasks containing the experimental waters, see Note, p. 185.

43—44 colonies per 1 c.c. were found in one flask (1 R, porcelain, Table IV) by ordinary plate cultivation, only 2—5 colonies in the same volume were found after the preliminary heating to 70° C. for 2 minutes. On comparing these results with those obtained by the same method in the case of the unsterilised waters, it will be seen that there is distinct evidence of the anthrax organs undergoing more rapid degeneration in the latter than in the sterilised waters. The degeneration is, however, in any case, only an extremely slow one.

There is hardly any difference to be found between the results yielded by the sterile waters kept at summer and winter temperatures respectively, but such slight difference as there is points rather to the degeneration of the anthrax being more retarded at the low than at the higher temperature.

One of these waters was also examined for virulence in October, or 7 months after infection with anthrax, and the following results obtained :—

Animal Experiment No. 9.—On October 15, 1892, 1 c.c. of water from the flask " 1 R, Thames water, porcelain-filtered, infected with anthrax, March 18, 1892," was subcutaneously injected into a white mouse. The mouse died within 4 days 17 hours. Anthrax bacilli were found in the spleen and by cultivation in gelatine. There was extensive œdema, and the spleen was not much enlarged.

Thus, in the water sterilised by filtration through porous porcelain the anthrax was still present after 7 months, in sufficient numbers for 1 c.c. to cause the death of the mouse, although from the comparatively slow action and non-typical symptoms it had apparently become somewhat attenuated.

I did not consider it necessary to make a corresponding experiment with the steam-sterilised Thames water, as, owing to the similarity between the results obtained by plate cultivation from both types of sterilised water, it appeared only reasonable to assume that their virulent effect would also be much the same.

5. *Vitality and Virulence of Anthrax in Thames Water (First Series) Exposed to Diffused Daylight.*

In all the experiments hitherto referred to, the waters were preserved in total darkness, the flasks containing them being placed in an incubator and refrigerator respectively. Further experiments were, however, made with the same waters, exposed to diffused daylight, at the ordinary temperature of the laboratory, and others, again, in which the flasks were exposed to direct sunshine.

The results of the experiments with Thames water (First Series), *exposed to diffused* daylight, are recorded in Table VI.

All the flasks employed in these experiments had been in the

refrigerator or incubator from the day of infection with anthrax (March 18, 1892) until March 25, 1892, from when they remained in a dark room until April 9, 1892, after which they were exposed to the diffused daylight in a room with a southern aspect.

An inspection of Table VI at once shows that the anthrax in the previously sterilised (porcelain and steam) Thames water survives this exposure to diffused daylight, nor does the number of colonies obtained on plate cultivation differ materially from that obtained from the corresponding flasks maintained throughout in the dark.

On the other hand, the degeneration of the anthrax in the unsterilised Thames water is distinctly more rapid in these flasks exposed to daylight than in those preserved in the dark. Thus, in the case of the unfiltered Thames water (daylight) the special method of examination revealed no anthrax from May 17, 1892, whilst in the same water, kept both in the incubator and refrigerator, anthrax was discovered by the same method on July 9, 1892.

The following experiments were made to test the virulence of the flasks which had been thus exposed to diffused daylight:—

Animal Experiment No. 5.—On October 8, 1892, 1 c.c. of water from the flask " 1 I, Thames water, unfiltered, infected with anthrax on March 18, 1892, and exposed to daylight since April 9, 1892," was subcutaneously injected into a white mouse. The mouse did not succumb, but is alive to the present time (November 11, 1892), or 32 days after the operation.

This result was to be anticipated, seeing that the corresponding flasks 3 I and 3 R, which had not been exposed to daylight, also failed to kill mice (see Animal Experiments Nos. 1 and 2).

Animal Experiment No. 3.—On October 7, 1892, 1 c.c. of water from the flask " 5 I, Thames water, paper-filtered, which had been infected with anthrax on March 18, 1892, and exposed to daylight since April 9, 1892," was subcutaneously injected into a white mouse. The mouse did not die, but is still alive, 33 days after the operation.

It will be remembered that a corresponding flask, 1 R, which had not been exposed to daylight did kill a mouse (see Animal Experiment No. 8), so that the virulence has in this case been reduced by the exposure.

Animal Experiment No. 4.—On October 7, 1892, 1 c.c. of water from the flask " 5 I, Thames water, porcelain-filtered, which had been infected with anthrax on March 18, 1892, and exposed to daylight since April 9, 1892," was subcutaneously injected into a white mouse. The mouse died within 6 days 20½ hours. The body exhibited extensive œdema ; the spleen was only slightly enlarged, but was found to contain anthrax bacilli both microscopically and by cultivation in gelatine.

Animal Experiment No. 10.—On October 15, 1892, 1 c.c. of water

from the flask " 5 I, Thames water, steam-sterilised, which had been infected with anthrax on March 18, 1892, and exposed to daylight since April 9, 1892," was subcutaneously injected into a white mouse. The mouse died within 4 days 17 hours; the body exhibited much œdema and the spleen was not very large; anthrax bacilli were detected in the latter both with the microscope and by cultivation in gelatine.

The contrast exhibited by the sterilised and unsterilised Thames water is thus again most striking in the case of these flasks exposed to daylight, for both the unfiltered and paper-filtered waters failed to kill, whilst the porcelain, filtered and the steam-sterilised waters were fatal to the mice into which they were injected. The lethal effect of both the latter, and especially of the porcelain-filtered water, accompanied by the non-typical symptom of only slight enlargement of the spleen, points again to an attenuation of the virus.

These results did not lead me to conclude, however, that the anthrax virus was necessarily quite extinct in these two unsterilised waters (viz., the unfiltered and paper-filtered Thames water), and I resorted, therefore, to the method before employed (see p. 194) of revivifying it by the addition of 5 c.c. of sterile broth to each of the two flasks in question. The flasks so treated were placed in an incubator at 37° C., and the following further experiments made with them :—

Animal Experiment No. 20.—On October 22, 1892, 0·5 c.c. of the water (to which broth had been added on October 15, 1892) in the flask " 1 I, Thames water, unfiltered, and infected with anthrax on March 18, 1892, exposed to daylight since April 9, 1892," was subcutaneously injected into a white mouse. The mouse died within 2 days 18 hours. The body exhibited extensive œdema and the spleen was much enlarged; the latter was found full of anthrax bacilli, the presence of which was confirmed by cultivation in gelatine.

Animal Experiment No. 17.—On October 18, 1892, 0·5 c c. of the water (to which broth had been added on October 15, 1892) in the flask " 5 I, Thames water, paper-filtered, and infected with anthrax March 18, 1892, exposed to daylight since April 9, 1892," was subcutaneously injected into a white mouse. The mouse died within 1 day 19 hours. Only few bacilli were found in the spleen, but more in the kidney; their presence was confirmed by gelatine cultivations from both organs.

These experiments show, then, *that in the flasks in question (unsterilised Thames water exposed to daylight), although the number of anthrax germs had been so far reduced that 1 c.c. would not kill mice, yet after nourishment with broth they were so revivified as to be fatal to these animals when injected in the same or even a smaller quantity.*

Table VI.—Anthrax in Thames Water. First Series of Experiments. Diffused Daylight Experiments. Infected March 18, 1892.

Date on which plates were poured.	Number of plate.	Particular flask and water employed.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies from 1 c.c. of water. Total.	Number of anthrax.	The flasks were exposed to diffused daylight from 9.4.92 onwards.
12.5.92	184	Unfiltered. 1 I *	4	c.c. 1/8	Too much liquefied	0	
	185	1 I	4	1/4		0	
	188	1 I	4	1/50	„	0	
	189	1 I	4	1/100	„	0	
17.5.92	180a	1 I	4	2	8	0	2 c.c. water + 1 c.c. broth, heated for 2 min. at 70° C
24.6.92	814	1 I	8	2	Too much liquefied	0	3 c.c. water + 1 c.c. broth, heated for 2 min. at 70° C.
	815	1 I	8	1/4	„	0	2 c.c. „ „ „
	818	1 I	4	1/15	15,300	0	
	819	1 I	4	1/175	17,850	0	
	822	1 I	6	3	8	0	
	823	1 I	4	2	8	0	
12.5.92	186	Paper filtered 5 I	4	1/8	Too much liquefied	0	
	187	5 I	4	1/4	„	0	
	190	5 I	4	1/50	„	0	
	191	5 I	4	1/100	„	0	
17.5.92	182	5 I	4	2	5	1	2 c.c. water + 1 c.c. broth, heated for 2 min. at 70° C.

* For explanation of the system of naming the flasks containing the experimental waters, see Note, p. 185.

Table VI—*continued.*

Date on which plates were poured.	Number of plates.	Particular flask and water employed. *	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies from 1 c.c. of water. Total.	Number of anthrax.	The flasks were exposed to diffused daylight from 9.4.92 onwards.
24.6.92	316	Paper filtered. 5 I	3	c.c. 2⅓	Too much liquefied	0	
	317	5 I	3	½	”	0	
	320	5 I	3	7/16	15,600	0	
	321	5 I	4	9/16	12,000	0	
	324	5 I	4	3⅜	4	0	3⅜ c.c. water + 1 c.c. broth, heated for 2 min. at 70°C.
	325	5 I	7	2⅓	2	1	2⅓ c.c. ” ” ” ”
12.5.92	194	Porcelain filtered. 5 I	15	1 1/7	22	22	Plate partially liquefied at edges, hence result only approximate.
	195	5 I	15	1/7	66	66	”
16.6.92	255	5 I	8	8	..	20	Contaminated with a micrococcus.
	256	5 I	8	1	..	43	” ” ”
12.5.92	192	Steamed. 5 I	15	1 1/7	27	27	” ” ”
	193	5 I	15	1/7	22	22	” ” ”
16.6.92	253	5 I	8	3	8,502	34	Contaminated with a bacillus.
	254	5 I	8	1	4,554	85	” ” ”

* For explanation of the system of naming the flasks containing the experimental waters, see Note, p. 185.

6. *Vitality and Virulence of Anthrax in Thames Water* (*First Series*) *Exposed to Direct Sunshine.*

The effect of direct sunshine is recorded in Table VII. The flasks were taken from the incubator and refrigerator respectively on March 25, 1892; they remained in a dark room from that day to April 9, 1892, and from then onwards they were exposed to as much sunshine as could be conveniently obtained, and which was approximately estimated in hours, although it is obviously very difficult to make any exact determination of the latter. The conditions of experiment are, of course, also much complicated by the fact that the temperature of the water so exposed was subject to very great variation, although it certainly never exceeded 32° C.

The results, which are very striking, are easily followed by reference to Table VII.

From the table it will be seen that in—

Unfiltered Thames water, anthrax was still alive on May 2, 1892, after 56 hours' sunshine, but extinct on May 12, 1892, after about 84 hours' insolation.

Paper-filtered Thames water, anthrax was almost extinct on May 15, 1892, after about 92 hours' insolation, and quite extinct on June 17, 1892, after about 151 hours' sunshine.

Thames water filtered through porcelain, anthrax was still alive on May 2, 1892, after about 56 hours of sun, but extinct on May 12, 1892, after about 84 hours' insolation.

Thames water sterilised with steam, anthrax was still alive on May 2, 1892, after about 56 hours', but dead on May 12, 1892, after about 84 hours' sunshine.

In consequence of the sunshine having destroyed the greater number of those water bacteria causing liquefaction of the gelatine, it was possible to incubate the plates for a long period of time, and thus in most instances to dispense with the special method of examination by preliminary heating already so often referred to.

The above results have only reference to the presence or absence of anthrax as revealed by cultivation, but experiments were also made on the virulence of these waters which had been exposed to direct insolation. Thus :—

Animal Experiment No. 30.—On November 2, 1892, 1 c.c. of the water from the flask " 4 I, Thames water, unfiltered, and infected with anthrax on March 18, 1892, exposed to 151 hours' sunshine," was subcutaneously injected into a white mouse. The mouse is still alive (November 14, 1892).

Animal Experiment No. 31.—On November 2, 1892, 1 c.c. of the water from flask " 4 I, Thames water steam-sterilised, and infected with anthrax on March 18, 1892, exposed to 151 hours' sunshine,"

Table VII.—Anthrax in Thames Water. First Series of Experiments. Sunlight Experiments, Thames Water infected with Anthrax, March 18, 1892.

Water.	Date when plates were poured.	Particular flask used.	Number of plate.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies in 1 c.c. of the water. Total.	Anthrax.	The flasks were exposed from 9.4.92 onwards.
Unfiltered	2.5.92	4 I	151	5	1 c.c.	Too much liquefied	0	56 hours' sunshine.
		4 I	152	5	1/10	700	0	
		4 I	155	8	1/25	Too much liquefied	0	
		4 I	156	8	1/250	10,000	550	Much importance cannot be attributed to the exact number in this case, as only 3 anthrax colonies were found on the plate, so that there is much inaccuracy possible in multiplying up to 1 c.c.
	12.3.92	4 I	196	15	1 3/5	Too much liquefied	0	84 hours' sunshine.
		4 I	197	15	3/5	„ 83,300	0	
		4 I	200	13	1/25	9,750	0	
		4 I	201	13	1/250		0	
	17.6.92	4 I	277	8	1	Not counted owing to gelatine liquefying	0	151 hours' sunshine.
		4 I	278	8	1/10	Too much liquefied	0	
		4 I	281	12	7/10	„ 33,550	0	
		4 I	282	8	1/25	500	0	3 c.c. water + 1 c.c. broth, heated for 2 min. at 70° C.
		4 I	286	12	3		0	
		4 I	237	14	1	33	0	1 c.c. „ „ „
Paper filtered	2.5.92	4 I	153	5	1	3,970	0	These were counted prematurely for fear of liquefaction. Only 1 and 2 anthrax colonies respectively were found on the plate, so that there is much chance of error in multiplying
		4 I	154	5	1/4	270	0	
		4 I	157	8	5/5	9,500	100	96 hours' sunshine.
		4 I	158	8	1/250	2,200	200	

Treatment	Date		Flask No.					Remarks
	15.5.92	4 I	198	15	1	7,630	2	92 hours' sunshine.
		4 I	199	15	⅘	4,500	5	
		4 I	202	13	12·5	19,150	0	
	17.6.92	4 I	279	8	1	11,900	0	3 c.c. water + 1 c.c. broth, heated for 2 min. at 70° C. ⎱ 161 hours' sunshine.
		4 I	280	8	1⅓	18,260	0	
		4 I	233	12	⅗	Too much liquefied	0	
		4 I	284	12	9⅗	41,500	0	
		4 I	289	14	3	1	0	1 c.c.
		4 I	289	14	1	4	0	
Porcelain filtered	2.5.92	4 I	161	7	2	Too much liquefied	0	56 hours' sunshine.
		4 I	162	7	⅟₁₁	18	7	
	12.5.92	4 I	206	13	1½	2	0	84 hours' sunshine.
		4 I	207	13	⅘	0	0	
	16.6.92	4 I	251	15	3	16	0	151 hours' sunshine.
		4 I	252	15	1	10	0	
Steamed	2.5.92	4 I	159	7	2	Too much liquefied	0	56 hours' sunshine.
		4 I	160	7	¼	16	4	
	12.5.92	4 I	204	13	1⅞	2	0	84 hours' sunshine.
		4 I	205	13	⅟₁₀	0	0	
	16.6.92	4 I	249	15	3	2	0	151 hours' sunshine.
		4 I	250	15	1	1	0	

Note.—Flasks up to 2.5.92 had received 56 hours' sunshine.

 ,, ,, 12.5.92 ,, ,, ,, 84 ,, ,,

 ,, ,, 15.5.92 ,, ,, ,, 92 ,, ,,

 ,, ,, 16.6.62 ,, ,, ,, 151 ,, ,,

 ,, ,, 17.6.92 ,, ,, ,, 151 ,, ,,

Temperature—Lowest 8° C.

     Highest 32 ,,

* For explanation of the system of naming the flasks containing the experimental waters, see Note, p. 185.

was subcutaneously injected into a white mouse. The mouse is still alive (November 14, 1892).

Animal Experiment No. 32.—On November 2, 1892, 1 c.c. of the water from flask "4 I, Thames water, porcelain-filtered, and infected with anthrax on March 18, 1892, exposed to 151 hours' sunshine," was subcutaneously injected into a white mouse. The mouse is still alive (November 14, 1892).

Thus, in all three cases, the water was non-virulent when injected to the amount of 1 c.c. ; this is, it will be observed, the first instance in which the sterilised waters infected with anthrax had become non-virulent. It was, however, obviously not to be necessarily concluded that the anthrax had become absolutely extinct in these waters, and in order to put this point to the test the flasks in question were each treated with 5 c.c. of sterile broth and incubated at 37° C., after which the following further experiments were made :—

Animal Experiment No. 36.—On November 9, 1892, 0·5 c.c. of the water (to which broth was added on November 7, 1892) from flask "4 I, Thames water, unfiltered, and infected with anthrax on March 18, 1892, exposed to 151 hours' sunshine," was subcutaneously injected into a white mouse. The mouse is still alive (November 14, 1892).

Animal Experiment No. 37.—On November 9, 1892, 0·6 c.c. of the water (to which broth was added on November 7, 1892) from flask "4 I, Thames water, steam-sterilised, and infected with anthrax on March 18, 1892, exposed to 151 hours' sunshine," was subcutaneously injected into a white mouse. The mouse is still alive (November 14, 1892).

Animal Experiment No. 38.—On November 9, 1892, 0·6 c.c. of the water (to which broth was added on November 7, 1892) from flask "4 I, Thames water, porcelain-filtered, and infected with anthrax on March 18, 1892, exposed to 151 hours' sunshine," was subcutaneously injected into a white mouse. The mouse is still alive November 14, 1892).

N.B.—These three mice, Nos. 36, 37, and 38, all lived much longer than November 14, 1802, so there can be no doubt that they escaped infection.

Thus, in these waters exposed to direct sunshine, the anthrax germs were completely destroyed and could not be revived by the addition of broth.

The destruction of anthrax spores by direct sunshine is a subject which has received the attention of a number of observers. Thus, Arloing (' Compt. Rend.,' vol. 100, 1885, p. 378, and vol. 101, p. 511) found that they were destroyed in two hours, whilst in subsequent experiments in which the spores were placed in broth maintained *at a temperature of* 4—11° C. by means of ice five hours' insolation

effected their destruction. Roux ('Ann. de l'Inst. Past.,' 1887, p. 445) again insolated the spores when dispersed in the aqueous humour of the ox-eye, and found them destroyed in from twenty-nine to fifty-four hours, whilst Pansini (' Rivista d'Igiene,' 1889) observed their destruction on potatoes in from four to five hours, in gelatine in from six to seven hours, and in broth in from thirty minutes to two hours. In all these experiments it will be seen that nutrient culture media were employed for the insolation, and that the spores were destroyed in a much briefer period of time than in my experiments, in which they were insolated in Thames water. This same phenomenon of *the spores of anthrax being more resistant to the action of sunshine in water than in ordinary culture materials* has also been observed by Straus ('Société de Biologie,' 1886, p. 473) and by Momont ('Ann. de l'Inst. Past.,'1892, p. 21), who both, however, appear to have made use of distilled water only.

II. Vitality and Virulence of Anthrax Spores in Thames Water (Second Series of Experiments).

Owing to the very small number of anthrax germs introduced into the water in the First Series of experiments, it was deemed advisable to carry out a Second Series in which a much larger number were inoculated into the several waters, whilst as a further modification and check, the infection was made with virulent anthrax from a totally different source to that employed in the First Series.

The infection was made as follows :—

50 c.c. of Thames water previously steam sterilised were placed in a small sterile stoppered bottle, and inoculated with 5 needle loops from an anthrax culture in glycerine agar of 3 weeks' age, and with 4 needle loops of another similar culture of $3\frac{1}{2}$ weeks' age. The water thus infected was then violently shaken, after which three portions of 5 c.c. each were measured with a sterile pipette into three flasks containing the three following waters respectively :—

(*a.*) Unfiltered Thames water (750 c.c.).
(*b.*) Thames water sterilised by filtration through porous porcelain (1000 c.c.).
(*c.*) Thames water sterilised by steam (1000 c.c.).

The waters thus infected were violently agitated to secure even distribution of the anthrax, after which each water was divided amongst a number of small flasks as follows :—

(*a.*) Infected unfiltered $\begin{cases} 3 \text{ small flasks for incubator (18—20° C.).} \\ 3 \quad \text{,,} \quad \text{,,} \quad \text{refrigerator (6—10° C.).} \end{cases}$
water ....

(*b.*) Infected porcelain- $\begin{cases} 3 \text{ small flasks for incubator.} \\ 3 \quad \text{,,} \quad \text{,,} \quad \text{refrigerator.} \end{cases}$
filtered water ..

(*c.*) Infected steamed $\begin{cases} 3 \text{ small flasks for incubator.} \\ 3 \quad \text{,,} \quad \text{,,} \quad \text{refrigerator.} \end{cases}$
water ....

The contents of these flasks were then from time to time submitted to bacteriological examination in the same way as in the First Series of experiments.

1. *Infected Unfiltered Thames Water (Second Series).*

The results of the experiments made with the unfiltered Thames water are recorded in Table VIII.

The plates poured on the day of infection (March 25, 1892) yielded about 15,000 colonies per cubic centimetre, of which about 12,000 were easily identifiable as anthrax.

The second series of plates were poured 4 days later (March 29, 1892), but those both from the incubator and the refrigerator flask were already so badly liquefied on the third day of incubation that it was impossible either to count or identify any anthrax colonies.

In consequence of this failure of the ordinary plate cultivation method, the special process for anthrax identification by preliminary heating was resorted to on April 13, 1892, for one of the refrigerator flasks, with the result that anthrax was easily recognised.

Ordinary plates were again poured some days later, on April 26, 1892, both from an incubator and a refrigerator flask, but in neither case could any anthrax colonies be found.

Ordinary plates were again poured on June 17, 1892, but only in one out of eight plates could any anthrax colonies be identified; nevertheless, when on the same day the special method by preliminary heating to 70° C. was adopted, a number of anthrax colonies were easily obtained both from the incubator as well as the refrigerator flask. This conclusively demonstrates the necessity of resorting to the special method of examination, and of not relying on that by ordinary plate cultivation.

The anthrax had thus retained its vitality in the unfiltered Thames water for 84 days, both when the water was maintained at a summer and a winter temperature respectively, although it was undoubtedly present in very much diminished numbers compared with those originally introduced.

Some of the flasks were finally examined on October 8, or 197 days after the original infection, and anthrax was still discoverable by the special method, although in slightly smaller numbers than on the *previous occasion.* By comparing these numbers with those obtained

in a corresponding experiment made on the same day (October 8) with the porcelain-filtered water (which was also orginally infected with about the same number of anthrax germs as the unfiltered water), it will be seen that the numbers have not diminished to nearly the same extent in the porcelain-filtered water as in the unsterilised water; in fact, in the porcelain-filtered water they have undergone hardly any reduction at all.

The following experiments were conducted in order to investigate the virulence of this unfiltered water in which plate cultivation had revealed the presence of living anthrax germs :—

Animal Experiment No. 21.—On October 23, 1892, 1 c.c. of the water in flask " 1 I, unfiltered Thames water (Second Series), infected with anthrax, March 25, 1892," was subcutaneously injected into a white mouse. The mouse died within 4 days 16 hours, anthrax bacilli being duly found in the spleen.

Animal Experiment No. 26.—On October 29, 1892, 1 c.c. of the water in flask " 2 I, unfiltered Thames water (Second Series), infected with anthrax, March 25, 1892," was subcutaneously injected into a white mouse. The mouse is still alive (November 14, 1892) 16 days after the operation, and therefore out of danger of anthrax.

Animal Experiment No. 25.—On October 29, 1892, 1 c.c. of water from the flask " 3 R, unfiltered Thames water (Second Series), infected with anthrax, March 25, 1892," was subcutaneously injected into a white mouse. The mouse died within 5 days 16 hours; the body exhibited extensive œdema, and the spleen was found full of anthrax bacilli, which was further confirmed by the gelatine cultivations prepared from that organ.

Animal Experiment No. 33.—This was performed on November 5, 1892, and was a repetition of Experiment No. 26. The mouse died within 2 days 4 hours. The spleen was much enlarged and found to contain anthrax bacilli, the characteristic growth being obtained in gelatine cultivations from the same organ.

Animal Experiment No. 34.—On November 5, 1892, 1 c.c. of water from the flask " 2 R, unfiltered Thames water (Second Series), infected with anthrax, March 25, 1892," was subcutaneously injected into a white mouse. The mouse died within 6 days 16 hours; the body exhibited much œdema; the spleen was considerably enlarged, and an abundance of anthrax bacilli were found in it.

We may conveniently also refer at this point to experiments made with some flasks resembling the above in all respects excepting that from July 23, 1892, onwards, when they were taken out of the incubator and refrigerator respectively, they had been exposed to diffused daylight, whilst the above flasks had been kept in the dark throughout. The results of the plate cultivations from these daylight flasks, which do not call for any special comment beyond that

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Table VII.—Anthrax in Thames Water. Second Series of Experiments. Unfiltered Thames Water, infected with Anthrax, March 25, 1892.

Date on which plates were poured.	Particular flask employed.	Number of plate.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies from 1 c.c. of water.		Remarks.
					Total.	No. of anthrax.	
25.3.92	∗	58	4	c.c. ½	12,820	10,780	This was the same sample of Thames water as was employed in the First Series of experiments.
		59	4	1/10	12,824	10,428	
		60	4	1/10	19,400	16,700	
		61	4	1/100	17,700	12,000	
29.3.92	1 I	70	8	1/10	Too much liquefied		
	1 I	71	8	1/10	„		
	1 I	74	8	1/100	„		
	1 I	75	8	1/100	„		
	1 R	72	8	½	„		
	1 R	73	8	1/10	„		
	1 R	76	8	1/100	„		
	1 R	77	8	1/100	„		
13.4.92	8 R	91	5	8	72	44	8 c.c. water + 1 c.c. broth heated for 2 min. at 70° C.
	8 R	92	5	1	60	24	1 c.c. „ „ „
	8 R	93	5	8	12	6	8 c.c. „ „ „
	8 R	94	7	1	42	8	1 c.c. „ „ „

Date	Flask	No.					Remarks
26.4.92	1 I	122	3		15,250	0	
	1 I	123	3		14,600	0	
	1 I	126	4		Too much liquefied	0	
	1 I	127	4		16,500	0	
	1 R	124	4		1,260	0	
	1 R	125	4		1,476	0	
	1 R	128	4		Too much liquefied	0	
	1 R	129	4		6,900	0	
17.6.92	1 I	265	3		7,344	0	
	1 I	266	3		7,548	0	
	1 I	269	4		9,400	0	
	1 I	270	4		8,250	0	
	1 R	267	8		5,028	0	
	1 R	268	8		4,990	0	
	1 R	271	4		6,250	0	
	1 R	272	4		4,800	1,200	Only 4 anthrax colonies were actually found on the plate, so that, of course, there is much room for inaccuracy in multiplying up to 1 c.c.
17.6.92	1 I	273	7	$3\frac{3}{11}$	82	28	$3\frac{3}{11}$ c.c. water + 1 c.c. broth heated for 2 min. at 70° C.
	1 I	274	7	$1\frac{1}{11}$	62	49	$1\frac{1}{11}$ c.c. water + 1 c.c. broth heated for 2 min. at 70° C.
	1 R	275	4	$2\frac{4}{8}$	12	7	$2\frac{4}{8}$ c.c. water + 1 c.c. broth heated for 2 min. at 70° C.
	1 R	276	4	$1\frac{3}{8}$	10	4	$1\frac{3}{8}$ c.c. water + 1 c.c. broth heated for 2 min. at 70° C.
8.10.92	2 I	412	5	3	21	8	3 c.c. water + 1 c.c. broth heated for 2 min. at 70° C.
	2 I	413	5	8	28	8	8 c.c. ,, ,, ,, ,,
	2 R	414	5	8	32	8	8 c.c. ,, ,, ,, ,,
	2 R	415	5	8	17	18	8 c.c. ,, ,, ,, ,,

* For explanation of the system of naming the flasks containing the experimental waters, see Note, p. 185.

Table VIII—*continued*.

Date on which plates were poured.	Particular flask employed. *	Number of plate.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies from 1 c.c. of water. Total.	No. of anthrax.	
					Diffused Daylight.		
					These flasks were exposed to diffused daylight on July 23, 1892.		
18.10.92	8 I	485	4	3	15	5	3 c.c. water + 1 c.c. broth heated for 2 min. at 70°C.
	8 I	486	4	3	Too much liquefied	.. 0	3 c.c. ,, ,,
	8 I	489	8	1/3	15,480	0	
	8 I	490	8	1/3	11,520	0	
	1 R	487	4	3	23	7	3 c.c. ,, ,, ,,
	1 R	488	5	3	81	17	3 c.c. ,, ,, ,,
	1 R	491	8	1/3	20,034	0	
	1 R	492	3	1/3	20,592	0	

Temp. of incub. on 22.7.92, 19° C.; on 20.9.92, 17° C.; on 16.10.92, 15° C.   Temp. of refrig. on 23.7.92, 9° C.; on 20.10.92, 15° C. The refrigerator was no longer supplied with ice after 23.7.92.

* For explanation of the system of naming the flasks containing the experimental waters, see Note, p. 185.

anthrax was found in both of them, are recorded at the foot of Table VIII. Thus:

Animal Experiment No. 18.—On October 21, 1892, 1 c.c. of the water in flask "3 I, unfiltered Thames water, infected with anthrax, March 25, 1892," was subcutaneously injected into a white mouse. The mouse died within 3 days 17 hours; the spleen was much enlarged, and although no anthrax bacilli could be found either in the latter or in the kidney with the microscope, gelatine cultivations prepared from the spleen developed the characteristic growths in due course, leaving no doubt that the animal had succumbed to anthrax.

Animal Experiment No. 27.—On October 31, 1892, 1 c.c. of water from the flask "1 R, unfiltered Thames water (Second Series), infected with anthrax, March 25, 1892," was subcutaneously injected into a white mouse. The mouse died within 2 days 18½ hours, anthrax bacilli being found in the spleen, and the characteristic growth obtained on gelatine cultivation.

These experiments with the unfiltered Thames water (Second Series) on being contrasted with those of the same water (First Series) show that the virulence was distinctly greater in the Second than in the First Series, for both incubator and refrigerator flasks of the Second Series, irrespectively of whether they had been kept in darkness or in the daylight, were sufficiently virulent to be fatal to mice. That the anthrax must have suffered a certain amount of attenuation is clear from the fact that one of the mice (Experiment No. 26) remained alive after receiving 1 c.c. of the water in flask "2 I," although a second mouse similarly inoculated succumbed. *This unquestionably greater virulence of the unfiltered Thames water (Second Series) is, doubtless, due to the much larger number of anthrax germs with which the water was infected in the Second than in the First Series.* It is particularly interesting that the mouse in Experiment No. 26 remained alive, because from the plate cultivations (see Table VIII) it is perfectly certain that in the 1 c.c. injected a number of living anthrax germs must have been present, and it is obvious, therefore, that their virulence must have been weakened by the long residence in the unsterilised water.

2. *Experiments with Sterilised Thames Water (Second Series).*

The results of the experiments made on Thames water sterilised by filtration through porous porcelain are recorded in Table IX, whilst those on the same water sterilised by steam are given in Table X. From these tables it will be seen that about 6000 anthrax organisms per cubic centimetre were introduced into the porcelain-filtered water, and about 8000 into the steamed water; whilst in Table VIII it was shown that about 12,000 were introduced into the

Table IX.—Anthrax in Thames Water. Second Series of Experiments. Porcelain Filtered Thames Water, infected with Anthrax, March 25, 1892.

Date when plates were Poured.	Particular flask employed.	Number of plate.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies from 1 c.c. of water. Total.	Anthrax.	Remarks.
25.3.92	*	58	4	c.c. $\frac{1}{1}$	Too much liquefied through anthrax colonies		
	..	54	4	$\frac{1}{1}$	6380	6380	
29.3.92	1 I	64	6		6294	6294	
	1 I	65	6		6058	6045	
	1 R	68	6		3600	3600	
	1 R	69	6		3074	3069	
18.4.92	1 R	103	8		2173	2159	$\frac{1}{1}$ c.c. water + 1 c.c. broth, heated for 2 min. at 70° C.
	1 R	104	8		2646	2640	$\frac{1}{1}$ c.c. ,, ,, ,, ,,
	1 R	95	5		318	318	$\frac{1}{1}$ c.c. ,, ,, ,, ,,
	1 R	96	5		408	402	$\frac{1}{4}$ c.c. ,, ,, ,, 90° C.
	1 R	97	5		21	0	$\frac{1}{4}$ c.c. ,, ,, ,, ,,
	1 R	98	5		12	12	$\frac{1}{4}$ c.c. ,, ,, ,, ,,
26.4.92	1 I	132	8		1028	1028	
	1 I	133	8		870	864	
	2 I	140	8		4760	4760	
	2 I	141	8		5592	5592	
	1 R	136	8		2628	2625	
	1 R	137	8		2430	2400	
	2 R	144	8		3172	3170	
	2 R	145	8		2921	2904	

Date	Flask	No.					Remarks
16.6.92	2 I	243	6	$\frac{10}{5}$	6180	6180	Contaminated with a micrococcus.
	2 I	244	6	$\frac{12}{5}$	5909	5909	„ „
	2 R	247	5	$\frac{10}{12}$	8615	8615	„ „
	2 R	248	8	$\frac{8}{12}$	8520	8520	„ „
11.7.92	2 R	349	4	$\frac{10}{5}$	2458	2458	3 c.c. water + 1 c.c. broth, heated for 2 min. at 70° C.
	2 R	350	8	$\frac{12}{5}$	2756	2587	2 c.c. „ „ „ „
	2 R	353	8	3	151	145	„ „ „ „
	2 R	354	8	2	137	128	„ „ „ „
8.10.92	8 R	416	5	8	92	92	3 c.c. water + 1 c.c. broth, heated for 2 min. at 70° C.
	8 R	417	5	8	104	104	3 c.c. „ „ „ „
	8 R	418	5	$\frac{12}{12}$	3630	3630	„ „ „ „
	8 R	419	5	$\frac{4}{12}$	3300	3300	„ „ „ „
26.10.92	2 I	528	5	$\frac{1}{4}$	4180	4180	
	2 I	529	5	$\frac{1}{4}$	4784	4784	
	8 R	532	4	$\frac{1}{4}$	8540	8540	
	8 R	533	4	$\frac{1}{4}$	4400	4400	

During the vacation the temperature of the incubator was, 22.7.92, 19° C.; 20.9.92, 70° C.; 16.10.92, 15° C.
„ „ „ refrigerator was, 23.7.92, 9° C.; 20.10.92, 15° C.
The refrigerator was no longer supplied with ice after 30.7.92.

Diffused Daylight Experiments.

These flasks were placed in diffused daylight on July 23, 1892.

Date	Flask	No.					Remarks
18.10.92	8 I	495	4	$\frac{11}{1}$	1512	1512	⎫ Plates were covered with moulds, and, therefore,
	8 I	496	4	$\frac{4}{1}$	1209	1209	⎬ uncountable.
	2 R	499	6	$\frac{4}{1}$			⎭
	2 R	500	6	$\frac{1}{12}$			

* For explanation of the system of naming the flasks containing the experimental waters, see Note, p. 185.

Table X.—Anthrax in Thames Water. Second Series of Experiments. Steamed Thames Water, infected with Anthrax, March 25, 1892.

Date on which plates were Poured.	Particular flask employed.	Number of plate.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	No. of colonies from 1 c.c. of water.		Remarks.
					Total.	Anthrax.	
25.3.92	*	51	4	c.c. 1/12	Too much liquefied through anthrax		
		52	4	4/12	8060	8060	
29.3.92	1 I	62	6	6/12	5280	4224	Contaminated with a micrococcus.
	1 I	63	6	7/12	8800	6600	,, ,,
	1 R	66	6	1/6	4020	4012	,, ,,
	1 R	67	6	1/6	4060	4026	
13.4.92	1 R	105	8	1/100	4080	4077	¼ c.c. water + 1 c.c. broth heated for 2 min. at 70° C.
	1 R	106	8	1/100	8822	8822	⅛ c.c. ,, ,, ,, ,,
	1 R	99	5	1/100	1200	1176	¼ c.c. ,, ,, ,, 90° C.
	1 R	100	5	1/100	1500	1494	⅛ c.c. ,, ,, ,, ,,
	1 R	101	5	1/10	18	18	¼ c.c. ,, ,, ,, ,,
	1 R	102	5	1/10	75	75	⅛ c.c. ,, ,, ,, ,,
							Compare effect of same treatment on porcelain-filtered and unfiltered infected waters.
26.4.92	1 I	130	8	1/8	Large number of colonies, including anthrax, but so unevenly distributed as to render accurate numeration impossible	..	Contaminated with a micrococcus,
	1 I	131	8	1/10	Ditto		
	2 I	133	8		3560	3551	,,

Date		No.					Notes
16.6.92	2 I	139	8		4318	4312	
	1 R	134	8		3818	3816	
	1 R	135	8		3978	3978	
	2 R	142	8		3397	3890	
	2 R	143	8		3969	3934	
	2 I	241	6		6300	6300	
	2 I	242	6		4875	4875	
	2 R	245	8		5250	5250	
	2 R	246	8		5917	5850	
11.7.92	2 R	347	4		3770	3770	3 c.c. water + 1 c.c. broth heated for 2 min. at 70° C.
	2 R	348	5		2835	2835	2 c.c. ” ” ”
	2 R	351	8		181	179	These experiments were performed to determine the diminution in the number of anthrax colonies by broth experiments.
	2 R	352	8		168	163	
26.10.92	1 I	526	5		..	..	Contaminated, and impossible to count anthrax colonies.
	1 I	527	5		3700	3700	” ” ”
	3 R	530	4		4648	4648	”
	3 R	531	4				”

Diffused Daylight.

Flasks placed in Diffused Daylight, July 23, 1892.

Date		No.				
18.10.92	3 I	493	6		3540	3540
	3 I	494	6		3240	3240
	2 R	497	5		2736	2736
	2 R	498	5		2976	2976

Temperature of incubator, 22.7.92, 19° C.; on 20.9.92, 17° C.; on 16.10.92, 15° C.
Temperature of refrigerator, 28.7.92, 9° C.; 20.10.92, 15° C.

* For explanation of the system of naming the flasks containing the experimental waters, see Note, p. 185.

unfiltered water. This greater impregnation of the unfiltered water was intentionally effected so as to heighten the contrast which it was anticipated would be presented at the close of the experiments. Thus on June 16, 1892, or 83 days from infection, the sterile waters still yielded several thousand anthrax colonies per cubic centimetre, whilst in the unfiltered water on the following day (June 17, 1892) only 1200 anthrax colonies could at most be detected. Or, again, comparing the results obtained by the special method of preliminary heating, on June 17, 1892, the unfiltered water yielded a maximum of 49 colonies per cubic centimetre; whilst on July 11, 1892, or nearly a month later, the porcelain-filtered water yielded by the same method 128—145 colonies per cubic centimetre, and the steamed water on the same day (July 11, 1892), 163—179 colonies per cubic centimetre.

There is thus again the most convincing proof that the degeneration of the spores of anthrax is more rapid in the unsterilised than in the sterilised water, whilst it is almost immaterial in this respect whether the latter is sterilised by steam or by filtration through porous porcelain, although there is some slight evidence that the steam-sterilised water is more favourable to the preservation of the anthrax germs than that which has been rendered sterile by filtration through porcelain.

As regards the influence of temperature on the preservation of anthrax, in the First Series of experiments the evidence apparently pointed in the direction of the degeneration taking place more rapidly at the summer than at the winter temperature; whilst in the Second Series of experiments the indications are uncertain, and I am of opinion, therefore, that the difference of temperature in question is probably a matter of little consequence in this respect.

Finally examining the results obtained in the last plate cultivations made in October, it will be seen that the anthrax germs were still present in the sterile waters (steamed and porcelain-filtered) in practically undiminished numbers, whilst in the unsterilised water their numbers were so much reduced that they could only just be still recognised by the special method of preliminary heating.

I made the following experiments in order to test the virulence of these infected sterile waters of the Second Series :—

Animal Experiment No. 6 —On October 12, 1892, 1 c.c. of water from the flask " 1 I, Thames water, steam-sterilised (Second Series), infected with anthrax on March 25, 1892," was subcutaneously injected into a white mouse. The mouse died within 2 days 21 hours ; the body exhibited much œdema ; the spleen was very much enlarged, and anthrax bacilli were fairly abundant in it, the gelatine cultures *also developing the characteristic growths* in due course.

Animal Experiment No. 7.—On October 12, 1892, 1 c.c. of water from the flask " 2 I, Thames water, porcelain-filtered (Second Series), infected with anthrax, March 25, 1892," was subcutaneously injected into a white mouse. The mouse died within 2 days 5 hours; the body exhibited much œdema, but the spleen was very small; anthrax bacilli were found in the latter, both with the microscope and by cultivation in gelatine.

From these experiments it is evident, therefore, that *the sterile Thames waters (Second Series) were still virulent nearly 7 months after their infection with anthrax, and the virulence as measured by the rapidity of their lethal effect was noticeably greater than with the corresponding waters of the First Series.*

III. EXPERIMENTS ON THE VITALITY AND VIRULENCE OF THE *Bacillus anthracis* AND ITS SPORES IN LOCH KATRINE WATER.

Experiments were made on the same lines with the water supplied to Glasgow from Loch Katrine.

The water was collected personally by myself, on July 6, 1892, at the Anderson's College, Glasgow; some of the water was drawn directly into sterile bottles, and submitted to ordinary plate cultivation within a few hours of its collection. The plates, after three days' incubation at 18—20° C., yielded 74 colonies per 1 c.c. of water.

As this is, as far as I am aware, the first record of the plate cultivation of such moorland water, I take this opportunity of also referring to the results obtained by me, for another purpose, in the plate cultivation of a number of samples of that portion of the Dundee water supply which is derived from the Loch of Lintrathen, a very similar source to Loch Katrine. All the samples were taken during the months of June and July, and one in October, during the present year, and were submitted to cultivation in about one hour from the time of collection. The results were as follows :—

Date of collection.	No. of days the plates were incubated at 18—20° C.	No. of colonies obtained from 1 c.c. of water.
22.6.92	4	110
23.6.92	4	149
30.6.92	3	290
2.7.92	3	94
4.7.92	3	114
11.7.92	3	279
17.7.92	3	177
21.7.92	3	77
29.7.92	3	155
18.10.92	3	260

Table XI.—Behaviour of Bacteria normally present in Loch Katrine Water. Third Series of Experiments.
Unfiltered Loch Katrine Water.

Date on which plates were poured.	Particular flask employed.	Number of plate.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies from 1 c.c. of water.		Remarks.
					Total.	Anthrax.	
8.7.92	*	341	4	c.c.	512	0	The Loch Katrine water, collected 6.7.92, yielded on the day of collection, 74 colonies per 1 c.c.; this water remained in stoppered bottles at about 12° C. until 8.7.92, when these experiments were begun.
		342	5		803	0	
		345	7		900	0	
		846	7		925	0	
12.7.92	1 I	371	2		30,700	0	
	1 I	372	2		44,970	0	
	1 I	375	2		44,980	0	
	1 I	376	2		49,500	0	
	1 R	373	2		13,800	0	
	1 R	374	2		13,248	0	
	1 R	377	2		14,600	0	
	1 R	378	2		16,250	0	
22.7.92	1 I	408	3	3	4	0	3 c.c. water + 1 c.c. broth heated for 2 min. at 70° C.
	1 I	409	3	2	5	0	2 c.c. ,, ,, ,, ,,
	1 R	410	2	3	39	0	3 c.c. ,, ,, ,, ,,
	1 R	411	2	2	62	0	2 c.c. ,, ,, ,, ,,
26.10.92	1 I	502	3		339	0	
	1 I	503	3		342	0	
	1 I	506	4		310	0	
	1 I	507	4		325	0	
	1 R	504	3		2,400	0	
	1 R	505	3		2,712	0	
	1 R	508	3		3,075	0	
	1 R	509	3		3,125	0	

During the vacation the temperature of incubator on 22.7.92 was 19° C.; on 20.9.92, 17° C.; on 16.10.92, 15° C.; on 20.10.92, 15° C. refrigerator on 23.7.92, 9° C.; on 20.10.92, 15° C.

It should be pointed out that neither the Loch Katrine nor the Lintrathen waters are submitted to filtration before delivery, and that these figures, therefore, indicate the bacterial life present in these waters as they come from the loch, excepting in so far as changes may occur in their passage through the mains.

The behaviour of these bacteria normally present in the Loch Katrine water, when the latter is kept at winter and summer temperatures respectively, was investigated, and the results are recorded in Table XI. From this it will be seen that in four days a very considerable multiplication had taken place, the increase in numbers being much greater in the case of the incubator (19° C.) flask than in that of the refrigerator (9° C.). After three months, however, the numbers in the incubator flask had fallen very much below those in the refrigerator flask, showing that the higher temperature leads to rapid multiplication followed by rapid decline, whilst at the lower temperature the increase and decrease are of a more gradual character.

As in the experiments on the behaviour of the anthrax bacilli in Loch Katrine water, the latter was used both in the natural condition and sterilised by filtration through porous porcelain; the water was also submitted to chemical analysis both in its natural state and after passage through the porcelain filter, with the following results :—

Results of Analysis expressed in Parts per 100,000.

	Loch Katrine water (unfiltered).	Loch Katrine water (filtered through porcelain).
Total solid matters	3·00	3·00
Organic carbon	0·195	0·220
,, nitrogen	0·015	0·030
Ammonia (free)...........	0	0·002
,, (albuminoid) ...	0·003	0·004
Oxygen consumed by organic matter, as measured by reduction of a solution of permanganate acting for three hours in the cold	0·116	0·140
Nitrogen as nitrates and nitrites..............	trace	trace
Total combined nitrogen ..	0·015	0·032
Chlorine	0·6	0·65
Temporary hardness	0	0
Permanent ,, ...	0·8	0·8
Total ,, ...	0·8	0·8
Remarks	clear and palatable	clear.

These analyses show the Loch Katrine water experimented with

to be of its usual character; as far as mineral ingredients are concerned, it is but little removed from distilled water; it contains, however, just about the same proportion of organic matter as Thames water, although the smaller yield of albuminoid ammonia and the larger amount of oxygen absorbed from permanganate show this organic matter to be qualitatively different. In point of fact, the organic matter in Loch Katrine water is almost exclusively of peaty origin, whilst that in Thames water, coming as it does from land which is under high cultivation, is derived from a variety of sources, both vegetable and animal.

The Loch Katrine water was also experimented with in three different states : (a) *in the natural condition unsterilised*, (b) *sterilised by filtration through porous porcelain, and* (c) *sterilised by steam.*

The method of infection was similar to that already described for Thames water; the sporiferous anthrax bacilli were taken from an agar-agar cultivation of four days' age. Eight needle loops full of the surface growth were introduced into 50 c.c. of sterile water, and thoroughly mixed by violently shaking for 15 minutes; 1 c.c. of this attenuation was then employed for infecting 750 c.c. of each of the three waters.

The waters, after infection, were distributed in a number of small flasks, which were then disposed of as follows :—

Loch Katrine water, unfiltered . {	3 flasks in refrigerator (6—10° C.).	
	3 „ incubator (18—20° C.).	
Loch Katrine water, filtered {	3 „ refrigerator.	
through porcelain .... {	3 „ incubator.	
Loch Katrine water, steamed .. {	3 „ refrigerator.	
	3 „ incubator.	

Gelatine plates were in each case poured on the day of infection, and also on several occasions subsequently, after the lapse of various intervals of time. The results of these examinations will be found in the following Tables XII, XIII, and XIV :—

Table XII.—Anthrax in Loch Katrine Water. Third Series of Experiments. Unfiltered Loch Katrine Water, Infected with Anthrax, July 8, 1892.

Date on which plates were poured.	Particular flask employed *	Number of plate.	Number of days plates were incubated.	Volume of water employed for plate cultivation	Number of colonies from 1 c.c. of water.		Remarks
					Total.	Anthrax.	
8.7.92	1 I	339	5	*c.c.*	6,330	5880	
	1 I	340	5	1/4	5,160	4580	
	1 I	343	5	1/8	5,600	4750	
	1 I	344	5	1/16	5,050	4650	
12.7.92	1 I	363	2	1/4	Too much liquefied	Anthrax too small to count	
	1 I	364	2	1/16	97,920	"	
	1 I	367	2	1/32	97,150	"	
	1 I	368	2	1/16	109,500	"	
	1 R	365	2	1/4	26,400	"	
	1 R	366	2	1/16	27,840	"	
	1 R	369	2	1/16	22,060	"	
	1 R	370	2	1/16	22,325	"	
22.7.92	1 I	403	8	3	1,224	1224	3 c.c. water + 1 c.c. broth heated for 2 min. at 70° C.
	1 I	404	3	2	1,044	1044	2 c.c. „ „ „
	1 R	405	2	3	3,432	3432	3 c.c. „ „ „
	1 R	406	2	2	4,002	4002	2 c.c. „ „ „

* For explanation of the system of naming the flasks containing the experimental waters, see Note, p. 185.

Table XII—continued.

Date on which plates were poured.	Particular flask employed.	Number of plate.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies from 1 c.c. of water.		
					Total.	Anthrax.	
18.10.92	1 I	469	5	c.c. 3	0	0	3 c.c. water + 1 c.c. broth heated for 2 min. at 70° C.
	1 I	470	5 and 6	3	2	0	3 c.c. „ „ „ „
	1 I	473	4	$\frac{3}{5}$	90	0	
	1 I	474	4	$1\frac{1}{7}$	77	0	
	2 I	475	6	$1\frac{3}{5}$	7	0	
	2 I	476	5	$1\frac{1}{5}$	96	0	
	1 R	471	5	3	325	325	Practically pure cultivation of anthrax. 3 c.c. water + 1 c.c. broth heated for 2 min. at 70° C.
	1 R	472	3 and 4	.3	...	Numerous typical anthrax colonies visible, but not far enough advanced to count.	3 c.c. water + 1 c.c. broth heated for 2 min. at 70° C.
	1 R	477	3	$\frac{5}{14}$	Too much liquefied	...	Anthrax noticed (microsc.), but too small to count.
	1 R	478	3	$1\frac{1}{5}$	1,862	...	„ „ „
	2 R	479	3	$\frac{5}{6}$	1,654	...	„ „ „
	2 R	480	3 and 4	$1\frac{1}{5}$	2,184	...	„ „ „

Diffused Daylight.

These Flasks were placed in Diffused Daylight on July 23, 1892.

Date		No.				3 c.c. water + 1 c.c. broth heated for 2 min. at 70° C.
						3 c.c. ,, ,, ,,
8.10.92	3 I	424	7	3	14	0
	3 I	425	7	3	7	0
12.10.92	3 I	464	5	1 6/3	24	0
	3 I	465	5	1 1/3	0	0
	3 R	466	6	1 5/3	4,140	3780
	3 R	467	6	1 1/3	6,072	5400
18.10.92	3 I	481	6	1 6/3	Too much softened	0
	3 I	482	6	1 1/3	360	
	3 R	483	6	1 6/3	5,806	5746
	3 R	484	6	1 1/3	5,472	5436

During the vacation the temperature of incubator on 22.7.92 was 19° C.; on 20.9.92, 17° C.; on 16.10.92, 15° C.

,, ,, refrigerator on 23.7.92 was 9° C.; on 20.10.92, 15° C.

* For explanation of the system of naming the flasks containing the experimental waters, see Note, p. 185.

Table XIII.—Anthrax in Loch Katrine Water. Third Series of Experiments. Porcelain-filtered Loch Katrine Water, Infected with Anthrax, July 8, 1892.

Date on which plates were poured.	Particular flask employed.	Number of plate.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies from 1 c.c. of water.		Remarks.
					Total.	Anthrax.	
8.7.92	1 I	333	5	c.c. $\frac{1}{4}$	4,385	4,385	
	1 I	334	5	$\frac{1}{2}$	4,200	4,200	
12.7.92	1 I	357a	5	$\frac{1}{6}$	10,368	10,368	Gelatine was very much softened, hence over-estimated.
	1 I	358a	5	$\frac{1}{13}$	7,680	7,680	
	1 R	361a	7	$\frac{1}{14}$	5,754	5,754	
	1 R	362a	4	$\frac{1}{5}$	5,358	5,358	One large mould on plate, and thus obliged to count earlier than others.
18.7.92	1 I	381	6	$\frac{1}{19}$	Uncountable	All anthrax	
	1 I	382	6	$\frac{1}{6}$	7,212	7,212	
	1 R	385	6	$\frac{1}{14}$	5,005	5,005	
	1 R	386	6	$\frac{1}{14}$	6,160	6,160	
8.10.92	1 R	427	6	3	3,332	3,332	Experiments to determine diminution in number of anthrax colonies. 3 c.c. water + 1 c.c. broth heated for 2 min. at 70° C.
	1 R	428	6	3	Too much softened to count	...	3 c.c. water + 1 c.c. broth heated for 2 min. at 70° C.
	1 R	429	6	$\frac{1}{4}$	8,364	8,364	
	1 R	430	6	$\frac{1}{4}$	8,400	8,400	

Diffused Daylight.

Flasks placed in Diffused Daylight on July 23, 1892.

11.10.92	2 I	441	6		4,315	4,315
	2 I	442	6		4,176	4,176
	1 R	445	6		7,233	7,233
	1 R	446	6		6,340	6,340
12.10.92	1 I	450	4		3,860	3,860
	1 I	451	4		2,928	2,928
	2 R	454	4		6,737	6,737
	2 R	455	4		6,279	6,279
12.10 2	3 I	458	7		6,575	6,575
	3 I	459	7		5,866	5,866
	3 R	462	7		11,088	11,088
	3 R	463	7		9,662	9,662

During the vacation the temperature of incubator on 22.7.92, 19° C.; on 20.9.92, 17° C.; on 16.10.92, 15° C.

,, refrigerator on 23.7.92, 9° C.; on 20.10.92, 15° C.

* For explanation of the system of naming the flasks containing the experimental waters, see Note, p. 185.

Table XIII.—Anthrax in Loch Katrine Water. Third Series of Experiments. Porcelain-filtered Loch Katrine Water, Infected with Anthrax, July 8, 1892.

Date on which plates were poured.	Particular flask employed.	Number of plate.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies from 1 c.c. of water.		Remarks.
					Total.	Anthrax.	
8.7.92	1 I	333	5	c.c. ⅘	4,385	4,385	
	1 I	334	5	1⅖	4,200	4,200	
12.7.92	1 I	357a	5	⅚	10,368	10,368	Gelatine was very much softened, hence over-estimated.
	1 I	358a	5	1⅗	7,680	7,680	
	1 R	361a	7	1⅘	5,754	5,754	
	1 R	362a	4	⅚	5,358	5,358	One large mould on plate, and thus obliged to count earlier than others.
18.7.92	1 I	381	6	1⅘	Uncountable	All anthrax	
	1 I	382	6	⅗	7,212	7,212	
	1 R	385	6	⅘	5,005	5,005	
	1 R	386	6	⅚	6,160	6.160	
8.10.92	1 R	427	6	3	3,332	3,332	Experiments to determine diminution in number of anthrax colonies. 3 c.c. water + 1 c.c. broth heated for 2 min. at 70° C.
	1 R	428	6	3	Too much softened to count	..	3 c.c. water + 1 c.c. broth heated for 2 min. at 70° C.
	1 R	429	6	⅘	8,364	8,364	
	1 R	430	6	⅘	8,400	8,400	

Date	Flask	No.				
11.10.92	2 I	441	6		4,315	4,315
	2 I	442	6		4,176	4,176
	1 R	445	6		7,233	7,233
	1 R	416	6		6,340	6,340
12.10.92	1 I	450	4		3,860	3,860
	1 I	451	4		2,928	2,928
	2 R	454	4		6,737	6,737
	2 R	455	4		6,279	6,279

Diffused Daylight.

Flasks placed in Diffused Daylight on July 23, 1892.

Date	Flask	No.				
12.10.'2	3 I	458	7		6,575	6,575
	3 I	459	7		5,866	5,866
	3 R	462	7		11,088	11,088
	3 R	463	7		9,662	9,662

During the vacation the temperature of incubator on 22.7.92, 19° C.; on 20.9.92, 17ᶜ C.; on 16.10.92, 15° C.;
„ „ refrigerator on 23.7.92, 9° C.; on 20.10.92, 15° C.

* For explanation of the system of naming the flasks containing the experimental waters, see Note, p. 185.

Table XIV.—Anthrax in Loch Katrine Water. Third Series of Experiments. Steam-sterilised Loch Katrine Water, Infected with Anthrax, July 8, 1892.

Date on which plates were poured.	Particular flask employed.	Number of plate.	Number of days plates were incubated.	Volume of water employed for plate cultivation.	Number of colonies from 1 c.c. of water.		Remarks.
				c.c.	Total.	Anthrax.	
8.7.92	1 I	331	5	¼	4,868	4,868	
	1 I	332	5	¼	5,133	5,133	
12.7.92	1 I	355a	7	¼	19,521	19,521	All possibly over-estimated through gelatine being much softened by anthrax colonies.
	1 I	356a	7	¼	23,004	23,004	
	1 R	359a	7	⅛	12,420	12,420	
	1 R	360a	7	1⁄12	10,282	10,282	
18.7.92	1 I	379	6	¼	Uncountable	All anthrax	Too much softened to count.
	1 I	380	6	¼	,,	,,	,, ,, ,,
	1 R	383	6	1⁄10	,,	,,	,, ,, ,,
	1 R	384	6	5⁄11	,,	,,	,, ,, ,,
11.10.92	1 I	409	4	¼	13,359	13,359	
	1 I	410	4	¼	10,950	10,950	
	2 R	443	4	⅛	13,840	13,840	
	2 R	444	4	1⁄12	15,016	15,016	
12.10.92	2 I	448	7	¼	10,890	10,890	Largely contaminated, but only anthrax colonies counted.
	2 I	449	7	⅛	13,266	13,266	
	1 R	452	4	⅛	29,426	29,426	Largely contaminated, and probably over-estimated. Results only approximate. Impossible to accurately estimate anthrax colonies.
	1 R	453	4	1⁄12	26,827	26,827	

Diffused Daylight.

Flasks placed in Diffused Daylight on July 23, 1892.

					12.10.92
3 I	456	7	4/6	16,632	16,632
3 I	457	7	5/15	16,742	16,742
3 R	460	7		24,064	24,064
3 R	461	7		21,120	21,120

During the vacation the temperature of incubator on 22.7.92 was 19° C.; on 20.9.92, 17° C.; cn 16.10.92, 15° C.

,, ,, refrigerator on 23.7.92, 9° C.; on 20.10.92, 15° C.

* For explanation of the system of naming the flasks containing the experimental waters, see Note, p. 185.

Turning in the first instance to Table XII, in which the results for the unsterilised water are recorded, it will be seen from the plates poured on the day of infection (July 8, 1892) that the sporiferous anthrax bacilli had been introduced to the amount of about 5000 per cubic centimetre, the water bacteria being present to the number of about 600 per cubic centimetre only. From Tables XIII and XIV it will be seen that the sporiferous anthrax bacilli had been introduced into the sterile waters in also just the same numbers as into the unsterile water, viz., about 5000 per cubic centimetre.

In the case of the unsterilised water, there was no difficulty in counting the anthrax colonies on the plates poured on the day of infection (July 8, 1892); but already, four days afterwards, the number of water bacteria had so greatly increased that the plates could not be preserved long enough for the proper development of the anthrax colonies, although they could still be just recognised as minute dots with a low power of the microscope. The multiplication of the water bacteria was greatest in the flask which had been kept at 18—20° C., although it was also very considerable in the one which had been in the refrigerator at about 9° C.

A fortnight (July 22, 1892) after the day of infection, anthrax was easily discoverable in large numbers by means of the special method of preliminary heating to destroy the water bacteria, there being three or four times as many colonies on the plates from the refrigerator flask as on those from the flask which had been kept at 18—20° C. This difference becomes still further accentuated later on, for on examining those flasks which had been continuously at the temperature (19° C.) of the incubator up to October 18, 1892, it was found that no anthrax could be demonstrated, whilst in those flasks which had been at the temperature of the refrigerator (9° C.) up to July 23, 1892, and up to 15° C. afterwards, a large number of anthrax colonies was obtained on cultivation. This contrast was presented both by those flasks which were kept continuously in the dark, as well as by those which had been placed in the daylight from July 23, 1892, onwards. From this it would appear that the spores of anthrax undergo markedly more rapid degeneration in the unsterilised Loch Katrine water at 20° C. than at 9° C. As will be seen presently, this striking phenomenon is not exhibited by the sterilised Loch Katrine water, in which there is little difference between the numbers of anthrax colonies obtained from the incubator and refrigerator flasks respectively.

It became, of course, particularly interesting to ascertain whether these remarkable differences between the incubator and refrigerator flasks would be maintained also in respect of virulence, and to determine this point the following direct experiments were made :—

Animal Experiment No. 11.—On October 16, 1892, 1 c.c. of the

water from the flask "1 I, Loch Katrine, unfiltered, infected with anthrax, July 8, 1892," was subcutaneously injected into a white mouse. The mouse is still alive (November 10, 1892), or 25 days after the operation.

Animal Experiment No. 22.—On October 23, 1892, 1 c.c. of water from the flask "2 I, Loch Katrine, unfiltered, infected with anthrax, July 8, 1892," was subcutaneously injected into a white mouse. The mouse is still alive (November 10, 1892), or 18 days after the operation.

Animal Experiment No. 19.—On October 21, 1892, 1 c.c. of water from the flask "1 R, Loch Katrine, unfiltered, infected with anthrax, July 8, 1892," was subcutaneously injected into a white mouse. The mouse died within 2 days 15 hours; the body exhibited extensive œdema; the spleen was much enlarged, and was found full of anthrax bacilli, the characteristic growth being obtained in gelatine cultivation.

Animal Experiment No. 23.—On October 23, 1892, 1 c.c. of water from the flask "2 R, Loch Katrine, unfiltered, infected with anthrax, July 8, 1892," was subcutaneously injected into a white mouse. The mouse died within 2 days 16 hours, and, although no bacilli could be microscopically detected in the spleen, their presence was revealed by gelatine cultivations made from that organ, thus leaving no doubt that the animal succumbed to anthrax.

Thus, the experiments made on mice with the unsterilised Loch Katrine water are in precise harmony with the results obtained by plate cultivation ; the two incubator flasks failed to kill, whilst the two corresponding refrigerator flasks were fatal to the mice, into which they were injected in the same quantity.

The same striking contrast was likewise obtained in the case of two similar flasks, which had, however, been exposed to daylight since July 23, 1892. Thus—

Animal Experiment No. 12.—On October 17, 1892, 1 c.c. of water from the flask "3 I, Loch Katrine, unfiltered, infected with anthrax, July 8, 1892, exposed to daylight since July 23, 1892," was subcutaneously injected into a white mouse. The mouse is still alive (November 10, 1892), or 18 days since the operation.

Animal Experiment No. 13.—On October 17, 1892, 1 c.c. of water from the flask "3 R, Loch Katrine, unfiltered, infected with anthrax, July 8, 1892, exposed to daylight since July 23, 1892," was subcutaneously injected into a white mouse. The mouse died within 2 days 18 hours; the body exhibited much œdema; the spleen was slightly enlarged, and anthrax bacilli were discovered in the latter, both by the microscope and by cultivation in gelatine.

Thus, in the case of the flasks subsequently exposed to daylight, also, the incubator flask proved harmless, and the refrigerator flask fatal, to the mice, into which they were respectively injected in equal quantity.

It was not, however, to be forthwith concluded, that the anthrax germs in these incubator flasks were necessarily extinct, and the endeavour was made, as in previous cases described above, to revive them, by the addition of broth to the waters. On October 29, 1892, therefore, 5 c.c. of sterile broth were accordingly added to each of the flasks " 1 I," and " 2 I," which were then placed in an incubator at 37° C., after which the following two experiments were performed :—

Animal Experiment No. 28.—On October 31, 1892, 0·5 c.c. of the water from the flask (to which broth had been added on October 29, 1892) " 1 I, Loch Katrine, unfiltered, infected with anthrax, July 8, 1892," was subcutaneously injected into a white mouse. The mouse is still alive (November 14, 1892), or 14 days after the operation, and, therefore, out of danger of succumbing to anthrax.

Animal Experiment No. 29.—On October 31, 1892, 0·5 c.c. of the water from the flask (to which broth had been added on October 29, 1892) " 2 I, Loch Katrine, unfiltered, infected with anthrax, July 8, 1892," was subcutaneously injected into a white mouse. The mouse died within 2 days 19 hours, anthrax bacilli being found in the spleen, and the characteristic growth obtained on gelatine cultivation.

Thus, in the case of flask " 1 I," the anthrax germs were extinct and could not be revived with broth; in the case of " 2 I," however, the addition of broth restored the virulence, so that some few anthrax germs must still have been alive in this flask.

Turning, in the next instance, to Table XIII, we find that on the day of infection (July 8, 1892) the porcelain-filtered Loch Katrine water yielded about 4000 colonies per cubic centimetre, whilst four days later (July 12, 1892) the number had risen to about 8000 in the flask kept at 18—20° C., whilst the refrigerator flask exhibited only a slight increase on the original number, a similar difference being observable again on the tenth day (July 18, 1892). From July 23, 1892, the temperature of the refrigerator was permitted to follow that of the room, and on October 8, 11, and 12, 1892, these refrigerator flasks yielded 6000—8000 anthrax colonies, whilst the incubator ones had fallen again to the original number of about 4000 per cubic centimetre.

Again, in the case of the flasks which had been exposed to the daylight from July 23, 1892, onwards, it was found, on October 12, 1892, that the refrigerator flask contained about 10,000, the incubator flask only 6000, anthrax germs.

It will be seen, therefore, that although there is evidence of the numbers being longer maintained at the low than at the high temperature, the contrast between the two is enormously less marked than in the case of the unsterilised Loch Katrine water.

In order to test the virulence of the porcelain-filtered water, I made the following experiments :—

Animal Experiment No. 15.—On October 17, 1892, 1 c.c. of water from the flask " 3 I, Loch Katrine, porcelain-filtered, infected with anthrax, July 8, 1892, exposed to daylight since July 23, 1892," was subcutaneously injected into a white mouse. The mouse died within 2 days 17 hours ; the body exhibited much œdema ; the spleen was slightly enlarged, and anthrax bacilli were found in it, both by the microscope and by gelatine cultivation.

The porcelain-filtered Loch Katrine water was, therefore, fully virulent more than three months after infection with anthrax.

Again, if we turn to Table XIV, in which the results with the steam-sterilised Loch Katrine water are recorded, we find that the multiplication after four days is more pronounced than in Table XIII, and, again, it will be seen that the multiplication is markedly greater in the flask kept at 18—20° C. than in the one kept in the refrigerator, although the latter also shows a multiplication of 100 per cent. In the later examinations, after the temperature of the refrigerator had been allowed to rise to that of the room (about 15° C.), the numbers in the incubator and refrigerator flasks became more equalised ; on the whole, the numbers were, at the final examination, greater in the refrigerator than in the incubator flasks, and this is markedly the case also in those flasks which, from July 23, 1892, onwards, were exposed to diffused daylight.

To test the virulence of the steam-sterilised Loch Katrine water, the following experiment was made :—

Animal Experiment No. 14.—1 c.c. of water from the flask " 3 I, Loch Katrine, steam-sterilised, infected with anthrax, July 8, 1892, exposed to daylight since July 23, 1892," was subcutaneously injected into a mouse. The mouse died within 1 day 20 hours ; the body exhibited extensive œdema ; the spleen was very much enlarged, and was found to be full of anthrax bacilli, which yielded the characteristic growth in gelatine cultivation.

Thus, the steam-sterilised Loch Katrine water was highly virulent more than three months after being infected with anthrax.

Conclusions to Part I.

The results obtained by me in the course of the above investigation on the vitality and virulence of anthrax spores in potable water may be summarised in the following statements :—

1. Three distinct series of experiments were made, viz. :—

Series I, in which Thames water collected above Staines was used, and a very small number of anthrax germs introduced into the water *(pp. 181—213).*

Series II, in which the same water was employed, but a much
larger number of anthrax germs introduced (pp. 213—225).

Series III, in which the water of Loch Katrine, taken as typical of
a moorland supply, was employed, and a large number of anthrax
germs introduced (pp. 225—239).

2. In all these three series of experiments the waters were infected
with anthrax from an agar-agar cultivation of such age as to ensure
the abundant presence of spores, so that the investigation deals
exclusively with the vitality and virulence of sporiferous anthrax
bacilli (pp. 184, 213, 228).

3. *In the sterilised waters both of the Thames and Loch Katrine* the
sporiferous anthrax bacilli maintain themselves in practically undi-
minished numbers for long periods of time—many months. In nearly
all cases, moreover, a distinct increase in the numbers was in the first
instance observed, which was followed in those cases in which the
experiments were extended over a sufficiently prolonged period by a
decline, which, however, in no case resulted in less than about one
half of the original number of anthrax spores being left in the water
after seven months. In the sterile Loch Katrine waters the anthrax
spores after upwards of three months were still two or three times
as numerous as in the first instance; indeed, there was more dis-
tinct evidence of multiplication in the sterile Loch Katrine waters
than in either of the two series of Thames water experiments (pp.
200—204, 219—225, 232—235, 238, 239).

As far as any difference could be established between the beha-
viour of the anthrax spores in these sterile waters at winter
(4—10° C.) and summer (18° C.) temperatures respectively, the
balance of evidence was on the whole in favour of the numbers
being longer maintained at the low than at the high temperature,
although the preliminary increase took place more rapidly at the
high temperature and was earlier followed by the subsequent decline
(pp. 204, 224, 238).

Practically no difference could be established between the beha-
viour of the anthrax germs in water *sterilised by steam* and by
filtration through porous porcelain respectively; in the case of the
Loch Katrine water there was indeed some evidence of the steam-
sterilised water being more favourable to the anthrax spores than
that which had been filtered through porcelain (pp. 201—203,
220—223, 232—234).

No effect could be traced to the influence of *diffused daylight* on
the behaviour of the anthrax spores in these sterile waters, the
numbers in daylight and in darkness being practically the same
(pp. 204—206, 208, 221, 223, 233, 235).

On the other hand, direct sunshine exerted a most marked effect,

for after 56 hours' insolation the number of anthrax spores was greatly diminished, and after 84 hours' exposure to solar radiation the presence of anthrax was no longer demonstrable by cultivation at all (pp. 209—213).

As regards the *virulence of the anthrax* in the sterilised waters of the Thames and Loch Katrine, the experiments which I have performed on mice conclusively prove that this is maintained over long periods of time—many months. In no single instance did the injection of 1 c.c. of these waters fail to kill the mouse, although the anthrax spores had been in the Thames water for upwards of seven months, and for upwards of three months in that of Loch Katrine. There is, however, unmistakable evidence of the rapidity of the lethal action of the anthrax depending on the number of spores present in the water. Thus in the First Series of Thames water experiments, in which only a small number of anthrax spores were present, the porcelain-filtered water was fatal in 4 days 17 hours, the steam-sterilised water which had been exposed to daylight was also fatal in 4 days 17 hours, and the porcelain-filtered water similarly exposed to daylight killed the mouse in 6 days 20½ hours; on the other hand, in the Second Series of Thames water experiments, in which a much larger number of anthrax spores were present in the water, the porcelain-filtered was fatal in 2 days 5 hours, and the steam-sterilised in 2 days 21 hours; and again in the case of the Loch Katrine water, in which a still larger number of anthrax spores were present at the time of the experiment, the porcelain-filtered killed in 2 days 17 hours, and the steam-sterilised in 1 day 20 hours (pp. 204—206, 224, 225, 239).

Of the sterilised Thames waters exposed to *direct sunshine*, neither the porcelain-filtered nor the steam-sterilised was fatal to mice, nor could their virulence be revived by the addition of broth to the water (pp. 209, 212).

4. *In the unsterilised Thames water*, both of the First and Second Series of experiments, the anthrax spores were indeed found to be still present in a vital state after many months, but in greatly diminished numbers, and thus furnishing the most striking contrast to their behaviour in the same water when sterilised either by steam or by porcelain filtration.

In the unsterilised Thames water of the First Series of experiments, anthrax was only just discoverable by the special method of cultivation which I devised for the purpose (see p. 185) 4 months after infection, so that it must have undergone great diminution in numbers during this period (pp. 195, 196, 198, 199).

This diminution in the number of anthrax spores was further established by the experiments on animals. Thus, when mice were subcutaneously injected with 1 c.c. of these unsterilised waters of the First Series, 7 months after the anthrax spores had been introduced,

the mice were not killed. That some few living anthrax spores were, however, still present in the water was proved by adding some broth to the water, which led to such a multiplication of the anthrax, that in the course of a few days the water thus treated became fatal on injection into mice. It is worthy of note in this connexion that one only of the unsterilised Thames waters (First Series) proved fatal without broth being added, and that this water had been filtered through Swedish paper before its infection with anthrax, so that it approached to some extent in its character the sterilised waters (pp. 193—194, 197, 200).

In the case of the unsterilised Thames water again it appears, as far as the evidence goes, that the anthrax spores are better preserved in the water at the winter than at the summer temperature (pp. 193, 197).

Daylight again appeared to be slightly unfavourable to the preservation of anthrax, the indications in this direction being more marked in the case of the First than in the Second Series of Thames water experiments (pp. 205, 218).

Sunlight, on the other hand, was most pronounced in its deleterious effect on the anthrax spores in the unsterilised Thames water. In the unfiltered water they were no longer discoverable by cultivation after 84 hours' sunshine, whilst in the paper-filtered water there were still a few present after 92 hours, but all had disappeared after 151 hours' insolation. These waters also which had been thus exposed to direct sunshine proved innocuous to mice, nor could their virulence be resuscitated by the addition of broth, clearly showing that the anthrax spores had perished to the last individual (pp. 209—212).

It is especially noteworthy in connexion with these results that they establish the remarkable fact that the anthrax spores, when immersed in water, are less prejudically affected by sunlight than when immersed in any of the ordinary culture materials. Thus, it has been shown by a number of observers that the anthrax spores suspended in broth and other culture materials are generally destroyed in the course of a few hours' exposure to sunshine, whilst in the above experiments the anthrax spores immersed in Thames water, both sterile and unsterile, resisted an insolation of upwards of 56 hours. This remarkable contrast between the behaviour of the anthrax spores in an aqueous and a nutrient medium respectively is also in accordance with the previous observations of Straus and of Momont, who both, however, appear to have experimented with distilled water only (pp. 212, 213).

In the unsterilised Thames water experiments of the Second Series, the conditions were different, inasmuch as the water was in the first instance infected with a much larger number of sporiferous anthrax bacilli. On this account, although a great diminution in the number

had taken place during the seven months' residence in the water, yet sufficient remained even then to be discoverable by cultivation, and to prove fatal to mice when 1 c.c. of the water was subcutaneously injected into them. Only in one instance did a mouse remain alive after receiving such an injection, and on repeating the experiment, the second mouse injected with the same water duly died of anthrax (pp. 214—219).

5. *In the unsterilised Loch Katrine water*, the behaviour of the anthrax spores was particularly remarkable. At the commencement of the experiment there were about 5000 anthrax germs and 500 other micro-organisms per cubic centimetre. These micro-organisms underwent, as was to be anticipated, very large multiplication, especially in that portion of the water which was kept at a summer temperature. Fourteen days after the commencement of the experiment cultivation still showed the anthrax to be abundant, but their number was markedly greater in that portion of the water which had been kept at winter than in that kept at summer temperature (p. 229).

On re-examination 3 months after the beginning of the experiment, the anthrax was absolutely undiscoverable by cultivation in the water kept at summer temperature, whilst it was still present in considerable, although greatly diminished numbers, in the water which had been kept at the lower temperature (p. 230).

The same difference was observed ir respect of virulence also, for in every case mice injected with the low temperature water died of anthrax, whilst those which received the same quantity of the higher temperature water remained alive. Indeed it was not until broth was added to this water kept at summer temperature that, of two flasks so treated, the one became virulent, whilst the other still remained innocuous (pp. 236—238).

It should be mentioned also that in this Loch Katrine water kept at the higher temperature (18° C.) the ordinary water bacteria became very much diminished in number after the preliminary multiplication referred to above (pp. 226, 227).

This remarkable bactericidal power of the unsterilised Loch Katrine water kept at 18° C. is doubtless due to the elaboration by the water bacteria of toxic products from the peaty organic matter present in this water, which products cause the destruction either of the spores or of the bacilli into which the anthrax spores will at this temperature gradually germinate. Moreover, the difference in this respect between the Thames water and that of Loch Katrine is to be sought for in the different nature of the organic matter present in these waters. The analyses show that quantitatively the organic matter (as measured by organic carbon and nitrogen) in both waters is almost exactly the same, but qualitatively they are very different, that in the Loch Katrine water being much less oxidised than that in Thames

water, as measured by the oxygen which they respectively absorb from permanganate. This doubtless means that the Loch Katrine organic matter has hitherto been a comparative stranger to bacterial life, whilst the organic matter of the Thames has been more fully exploited by the micro-organisms which are more abundant in its waters. It is evident, however, that the Loch Katrine organic matter *per se* is not possessed of bactericidal powers at the higher temperature, for in the sterilised Loch Katrine waters at that temperature the anthrax spores underwent no such destruction (pp. 182, 227, 232 —235, 238).

Part II.

"Experimental Investigations on the Behaviour of *Bacillus anthracis* in Water." By Professor MARSHALL WARD, D.Sc., F.R.S., assisted by G. E. CARTWRIGHT WOOD, M.D., B.Sc.[*]

It is obvious that some of the questions raised in our Report can only be settled by experimenting directly with the freshly collected water, and, since we selected a definite type of Thames water for our work, it was necessary to determine the main points in the natural history of this water itself, and to employ it directly for cultures.

Some of the principal results are submitted as follows :—

Bacteriological Examination of the Thames Water in its Natural Condition.

Preliminary.

On January 21, 1892, three samples of Thames water were submitted to examination immediately after collection, to obtain an answer to the question, Does the Thames water selected for inquiry contain bacteria at the moment of collection ?

Plate cultures were made in the usual way, by dropping known quantities, so many drops from a pipette containing 1 c.c., and known to emit so many drops per 1 c.c., of the water into gelatine melted at 30° C.

Sample I gave the following results :—

(*a.*) 1-drop plates incubated 5 days at 15° C. (drop = $\frac{1}{23}$ c.c.) gave an average of 2 colonies per plate = 46 bacteria per 1 c.c.

(*b.*) 3-drop plates in 7 days gave an average of 8 colonies per plate = 61 bacteria per 1 c.c.

(*c.*) 9-drop plates in 5 days gave an average of 8 colonies per plate = 10 bacteria per 1 c.c.

So far, it was clear that the river water contains *some* bacteria, 10 to 61 per c.c., which develop on gelatine. These were of several kinds, and developed at different rates, and pure cultures of the different forms were isolated for future reference, as it was part of our object to familiarise ourselves with the normal bacterial flora of the river.

Samples II and III were similarly examined, and with similar results, which need not be detailed here.

[*] All experiments on animals have been made by Dr. Cartwright Wood.

Series A.

Table A.—Thames Water in Natural State—Cultures made forthwith.

Dates on which water was collected.	Date of making plate.	Condition of water.	Number of days incubated.	Temperature, degrees C.	Number of drops used for plate.	Number of drops per 1 c.c.	Actual number of colonies on plate.	Calculated total number of bacteria per 1 c.c.	Remarks.
21.3.92	21.3.92	Fresh from river and untreated.	3	20—22°	1	24	29	696	
,,	,,	,,	3	,,	1	25	89	975	
,,	,,	,,	3	,,	3	24	104	832	
,,	,,	,,	3	,,	3	22	98	719	
,,	,,	,,	4	12—15	6	25	about 200	?	The plates liquefied so rapidly, it was impossible to count the numerous colonies accurately.
,,	,,	,,	4	,,	9	22	?	?	
20.6.92	20.6.22	,,	3	20	1	35	7	245	
,,	,,	,,	3	,,	6	30	100	500	
10.12.92	10.12.92	,,	2	20	2	25	69	862	
,,	,,	,,	,,	,,	4	25	200	1250	Many liquefying.
,,	,,	,,	,,	,,	9	25	390	1063	

In all cases the Thames water experimented with contains an appreciable, but not necessarily large, number of living bacteria, capable of germination and growth on gelatine plates. As will be seen later on, the above numbers are very small, no doubt owing to the low temperature of the water, and the promptness of the cultures direct from the river; moreover, the number per 1 c.c., as shown by gelatine plate-cultures, is to a great extent dependent on the temperature of incubations.

Series A.

In this series of experiments I confined my attention to the numbers of bacteria actually present in the Thames water when collected. The method followed was the usual one of carefully collecting the samples in sterile flasks, or occasionally in sterilised vacuum tubes drawn to a point which is broken under the water by forceps : these being heated and the glass point re-sealed in a spirit lamp at the river itself.

In no case given was the water allowed to stand more than a couple of hours or so, and then at low temperatures, and in some cases (employed as checks on the others) the plates were actually made within half an hour of collecting.

The method of making the plates was also the well-known one, and does not need description here; I employed round or square Petri's dishes in all cases.

Examples illustrating the results of these examinations are given in Table A.

Table A shows very clearly that *the number of bacteria actually present in the Thames water at the moment of collecting is not large*, for an open river, though differences appear to exist in June as contrasted with March and December as regards the numbers. I do not propose to go into these differences at present, however, since they are not striking, but it is worth noting that the experience of previous observers favours the supposition that monthly differences in the total number of bacteria of rivers are to be expected.[*]

Of course the point could only be decided by continuous observation, which I think should be carried out. It is, perhaps, not without interest to note that, so far as my few observations on this point go, they bear out the conclusions of Miquel,[†] that there are more bacteria in the river in March than in June, and more in December than in either of these months, a fact probably correlated with the surface drainage and rain washings. I repeat, however, that

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[*] See, for instance, Miquel, 'Manuel pratique d'Analyse Bactériologique des Eaux,' 1891, pp. 128—146, and the literature on rivers in our 1st Report.

[†] *Loc. cit.,* pp. 131—138.

my observations on this particular point are only by the way, and much too few for any valuable conclusions on the wider question.

As regards the species or forms met with, I defer their discussion for the present; suffice it to say that we carefully isolated and tabulated the different forms in order to experiment with them afterwards (see p. 285), and that a considerable number of the individuals (not species) were rapidly liquefying forms, rendering the keeping of the plates difficult.

Series B.

This series, part of a more extensive set of experiments to be referred to later, is calculated to show the kind of changes as regards the number of normal water bacteria, undergone by the river-water on standing. It should be regarded, therefore, as supplementing the results in the last table.

In each case the same procedure was adopted, and of course the same care in collecting the samples and making the plates, &c.

The collected water was placed in properly sterilised and plugged flasks, and allowed to stand, at the temperature given, undisturbed for a number of days.

Plates were made forthwith to determine the initial numbers of bacteria per 1 c.c. of the water, and then samples taken every twenty-four or forty-eight hours, for several days, to determine whether any, or what, increase or decrease in the total numbers had occurred in the interval.

It was to be expected, from the publications of others and from our own experience, that such increase would occur, and my preliminary experiments showed that in this case the increase is very great. Consequently we had to take precautions against having our plates too over-crowded with colonies, and this we did by adding pure distilled water to each sample taken for analysis in such quantities that the bacteria in 1 c.c. of the original water were distributed through 20 c.c., and making the plates from the diluted sample.

It is true this method involves the risk of killing some of the bacteria; but the results show that the numbers obtained are very large, nevertheless.

It is also true that the dilution method introduces a further source of error in compelling us to multiply the ascertained result—always a dangerous process in statistics. However, we have been unable to see any way out of this difficulty, and have relied rather on the general results expressed in the tables than on the actual numbers, which it is evident must be looked upon as approximations only.

SERIES B.

Table B (I).—Thames Water in Natural State. Cultures made forthwith and after standing.

Date of collecting water.	Date of making plate.	Condition of water.	Number of days incubated.	Tempera-ture, C.	Quantity used for plate.	*Diluted or not.	Number of colonies counted on plate.	Calculated total number of bacteria per 1 c.c.	Remarks.
12.5.92	12.5.92	fresh from river	3	18°—20	c.c. 1/16	not	6	175	
,,	13.5.92	,,	8	,,	1/16	,,	55	225	
,,		standing 24 hours at 20° C.	8	,,	1/4	diluted 20 times	1200 to 1600	120,000 to 860,000	100 squares contained (averaged by counting several carefully in different parts of the field) about 12 to 16 colonies each. Maximum and minimum chosen.
,,	,,	,,	3	,,	1/4	,,	4800	160,000	The 1/4 plate covered 20 squares, averaging 60 colonies per square.
,,	14.5.92	standing 48 hours at 20°	2	,,	1/4	,,	2160 to 3240	216,000 to 324,000	18 squares per 1/4 plate gave from 80 to 45 colonies per square.
,,	,,	,,	2	,,	1/4	,,	4800 to 5800	160,000 to 193,333	15 squares per 1/4 plate gave 80 to 90 colonies per square.
,,	16.5.92	standing 96 hours at 20°	2	,,	1/4	,,	2560 to 3200	256,000 to 320,000	16 squares per 1/4 plate = 40 to 50 colonies each.
,,	,,	,,	2	,,	1/4	,,	3200 to 3840	160,000 to 192,000	16 squares per 1/4 plate = 50 to 60 colonies per square.
,,	18.5.92	standing 144 hours at 20°	2	,,	1/4	,,	150	15,000	
,,	,,	,,	2	,,	1/4	,,	200	10,000	

* Diluted with sterile distilled water.

SERIES B.

Table B (II).—Thames Water in Natural State. Cultures made forthwith and after standing.

Date of collecting water.	Date of making plate.	Condition of water.	Number of days incubated.	Temperature, C.	Quantity used for plate.	*Diluted or not.	Number of colonies counted on plate.	Calculated total number of bacteria per 1 c.c.	Remarks.
20.6.92	20.6.92	fresh from river	8	18°—20	1 c.c.	not	7	245	Used 1 drop from a pipette which = 35 drops per 1 c.c.
,,	21.6.92	,, standing 24 hours at 18—20° C.	3	,,	1/5	diluted 20 times	100	500	
			3	,,	1/5	,,	125	12,500	
,,	22.6.92	,, standing 48 hours at 18—20° C.	3	,,	1/5	,,	280	9,333	In this case we found that 60 squares averaged 70 colonies each.
			3	,,	1/5	,,	4200	420,000	
,,	,,	,,	3	,,	1/5	,,	6000	200,000	100 squares averaged 60 colonies each.
,,	23.6.92	standing 72 hours at 18—20° C.	2	,,	1/5	,,	7000 to 8000	700,000 to 800,000	100 squares averaged 70 to 80 colonies each.
,,	,,	,,	2	,,	1/5	,,	uncountable	..	Partly owing to the enormous number, and partly to liquefaction, it was impossible to estimate.
,,	24.6.92	standing 96 hours at 18—20° C.	1	,,	1/5	,,	100,000?	1,000,000?	100 squares contained an average of 100 colonies each.
,,	25.6.92	standing 120 hours at 18—20° C.	3	,,	1/5	,,	7000 to 9000	700,000 to 900,000	100 squares contained an average of 70 to 90 colonies each.

* Diluted with sterile distilled water.

If we take the average numbers in the ninth column of Tables B (I) and B (II), it is clear that *a very rapid rise in the numbers occurred during the first twenty-four hours, and continues during the second day, and even to the third or fifth, and then comes a fall, slight at first, and then rapid.* Now, without insisting too closely on the numbers—indeed, we expressly desire to emphasize the fact that they can only be more or less approximate, from the nature of the case—it is interesting to note how closely the general result compares with the experience of other observers, working with the waters of rivers, &c., in other parts of the world. For the sake of this *general* comparison, I append our own averages and those of one or two other workers in the annexed tabular *résumé*, where the averages are taken in round numbers as approximations.

Numbers of Bacteria per 1 c.c. of Water.

Source of water.	Number of hours the water had stood at about 20° C.										
	0—2	24.	48.	72.	96.	120.	144.	10 days.	20 days.	30 days.	
Thames....	200	200,000	445,000	..	232,000	..	12,500	..	..	..	In May (Table B, I).
„	872	10,400	300,000	750,000	1,000,000	800,000	..	..	..	..	In June (Table B, II).
„	1065	83,900	337,500	12,500	18,100	8,250	..	22·5	..	..	In December.
Vanne......	125	38,000	125,000	590,000	..	..	..	..	..	..	Miquel, *loc. cit.*, p. 14.
„	71	71,000	..	..	1,070,000	..	..	..	..	..	}
Well water.	1990	*18,660	..	†26,100	..	‡37,000	..	..	§4,700	..	Meade Bolton, 'Zeit. f. Hyg.,' 1886, p. 76.
„	143	12,457	..	328,543	..	..	..	||233,452	¶17,436	..	Cramer. See Hueppe, 'Schilling's Journ.,' 1887, p. 43.

* Is given as 24—36 hours.   † Given as 2—4 days.   ‡ Given as 5—10 days.
§ Given as 20—30 days. || Given as 8th day.   ¶ = 17th day.

Experiments to test the Influence of Temperature on the Changes under-gone by the Normal Bacteria of Standing Thames Water.

To determine whether the effect of temperature on the increase of the bacteria of standing Thames water is very marked, the experiments summarised in Table *x* were carried out.

Two 1-litre flasks of the water were collected and at once carefully analysed, with the result that the Thames water collected on December 10 contained, on the average, 1065 bacteria per 1 c.c.

The flasks were then placed in the dark, one at 12° C., the other at 20° C., and examined periodically, with the results tabulated.

The column of remarks sufficiently denotes the behaviour noticed.

Table *x.*—Changes undergone by Thames Water on standing at 12° C. and at 20° C. in December.

Number of days flask stood.	Temperature at which flask stood.	Date of making plate.	Number of days plate was incubated at 20° C.	Quantity of water used for plate.	Diluted or not.	Number of colonies found on plate.	Calculated number of bacteria per 1 c.c. original.	Remarks.
0	°C.	Dec. 10	2	c.c.	Not	69	982	Average 1065, of which 881 were already evident as liquefying forms.
0	"	"	2	1	"	300	1,250	
0	"	"	2	1	"	380	1,083	
1	20	Dec. 11	2	1	"	5,000	125,000	Average 83,333, of which about 100 were already liquefying badly.
1	"	"	3	1	"	5,000	41,666	
1	12	"	2	1	"	70	1,750	Average 2858, of which 40 were badly liquefying forms.
1	"	"	3	1	"	500	4,166	
2	20	Dec. 12	2	1	"	17,500	437,500	Average 337,500, of which very many were badly liquefying.
2	"	"	2	1	"	19,000	287,500	
2	12	"	2	1	"	1,950	12,916	Average 14,999 — many badly liquefying.
2	"	"	2	1	"	2,050	17,083	
8	20	Dec. 13	2	1	"	..	..	Plate spoilt by over-heating, therefore must accept the second plate as average, though there also the colonies were retarded by over-heating the plate.
8	"	"	2	1	"	510	12,500	
8	12	"	2	1	"	24,000	600,000	Average 675,000, of which a large proportion liquefy.
8	"	"	2	1	"	30,000	750,000	
4	20	Dec. 14	2	1	"	310	23,250	Average 18,187, of which only about 500 are distinctly liquefying.
4	"	"	3	1	"	175	13,125	
4	12	"	2	1	"	5,500	412,500	Average 656,250, of which at least 15,000 are liquefying.
4	"	"	3	1	"	18,000	900,000	
5	20	Dec. 15	2	1	"	40	6,000	Average 8250, the liquefying forms much fewer.
5	"	"	2	1	"	70	10,500	

5	12	"	2	±	;;	600	90,000	Average 60,000, with 1500 liquefying forms.
5	;;	"	2	±	;;	200	30,000	
7	30	Dec. 17	2	±	;;	29	725	Of which 100 liquefied.
7	12	"	3	±	;;	850	21,250	Of which 500 liquefied.
8	20	Dec. 18	2	±	;;	6	150	None liquefying.
8	12	"	2	±	;;	450	11,250	Of which 500 liquefy.
10	20	Dec. 20	2	±	;;	27	225	Of which 25 liquefy.
10	12	"	2	±	;;	198	4,900	Of which 375 liquefy.
18	20	Dec. 28	2	±	;;	..	..	Spoilt by moulds.
18	12	"	2	±	;;	62	456	Of which 25 liquefy.
18	20	Dec. 28	3	±	;;	105	875	N.B.—Only five or six showed on second day; the temperature rose to 25° C.
18	12	"	2	±	;;	286	1,966	

The results seem to show conclusively that *the maximum number is not only higher at the higher temperature, but that it is attained more rapidly.* That this is, at least in part, due to the bacteria being enabled to multiply and diffuse themselves through the liquid more rapidly, before the available oxygen and food materials are diminished, seems an obvious conclusion; though I do not believe that these factors alone explain the phenomenon.

Experiments with the Vegetative Bacilli *of Anthrax in Thames Water.*

It is necessary to know, if possible, whether the living vegetative bacilli of anthrax can survive immersion in such waters as we have experimented with, and then to see if they can multiply therein: that spores can withstand such immersion has long been known, and we gave very full particulars on this point in our First Report,[*] but the evidence regarding the vegetative bacilli is somewhat conflicting, and consequently I have devoted attention especially to this point. The difficulties are decidedly great. In the first place it is not easy to obtain spore-free bacilli, and it will be objected that in some of the following cases it is not certain that my material was absolutely spore-free; this cannot be gainsaid, but it can at least be claimed from the experiments that, while they do not absolutely settle the question whether the vegetative bacilli can or cannot multiply in Thames water, they do show that, if such bacilli obtain access to the water and form spores in it, they are very tenacious of life and difficult to exterminate.

Preliminary.

On January 28, 1892, a sterilised $\frac{1}{2}$-litre flask was charged with 25 c.c. of Thames water, fresh from the river, and inoculated with a large charge of a potato cultivation of a normal anthrax grown at 16° C., and devoid of spores, so far as could be ascertained. We employed a potato culture in order to introduce as little nitrogenous food material as possible into the water, and chose a tube grown at a relatively low temperature (16° C.) to try and prevent the precocious development of spores.

Five $\frac{1}{2}$-litre flasks were then charged each with 25 c.c. of the freshly-collected Thames water, and inoculated each with 1 c.c. of the above infecting fluid. The flasks were marked A, B, C, D, and E.

Flask A was selected for periodic examination, to obtain a preliminary answer to the question, Can *Bacillus anthracis* maintain itself alive at all in Thames water? Pipettes were selected to drop 25 drops to the 1 c.c.

After standing 24 hours at 20° C., we made a series of plates (on

[*] *See* 'Roy. Soc. Proc.,' vol. 51, 1892, pp. 219 and 268.

January 29) with 1, 3, and 9 drops each respectively, and examined next day. The annexed example is selected.

1-drop plate = 5 colonies = 125 per 1 c.c. *No anthrax.*
3 „ = 67 „ = 558 „ „
9 „ = 260 „ = 720 „ „

This seemed to show that the vegetative bacilli rapidly disappear from the water, a result apparently in accordance with the experience of several previous observers.

A liquefying bacillus was common on the plates, however, and prevented our keeping them long enough to determine whether anthrax was really absent, or merely slower in development than the rest of the organisms.

A new set of plates were made from Flask A on January 30, *i.e.*, the flask having stood 48 hours at 20° C.

1-drop plate = 107 colonies = 2675 per 1 c.c.,

while plates with 3 and 9 drops respectively liquefied so rapidly that we could make no determinations of the numbers.

No anthrax colonies were found to develop in the time, and similar results were obtained next day, the flask having then stood 72 hours

The 1-drop plate = 320 colonies = 7900 per 1 c.c.

On February 2, the flask having stood 110 hours, a further set of plates were prepared, but the colonies developed were so numerous that we could not estimate them. On some of the plates, however, very small anthrax colonies appeared, and even in relatively large numbers. On the whole, this preliminary examination convinced us that such plates may fail to show anthrax colonies, because the normal water bacteria present develop so rapidly, and in such abundance, that the anthrax has no chance, especially if bad liquefying forms are present. They also showed us that the water forms increase in numbers, day by day, as the water stands.

As our further experiments show, *the conclusion that the anthrax died in these flasks is quite unwarranted; its persistence was due to the formation of spores;* but at the stage here reached that was a question to be inquired into.

Series B'.

This series was also designed to see whether virulent normal anthrax was capable of living as bacilli, and multiplying in the Thames water, either untouched or rendered sterile by filtration through porcelain or by heat, or if it passes over into spores in the water. In this series we boiled the Thames water for two hours.

The anthrax employed was a very virulent one, and known as "Edinburgh Cow A," grown on agar for thirty hours at 30° C. Eight flasks were used, and divided into four pairs; each flask received 25 c.c. of the water to be examined, and a large charge of anthrax—1 c.c. of the infecting fluid, which contained chiefly, if not entirely, bacilli.

Two flasks were charged with the Thames water in its crude state, and not infected at all.

Two were charged similarly with the crude water forthwith infected with anthrax.

Two were charged with the Thames water forthwith filtered through porcelain, and at once infected.

Two were charged with the boiled Thames water infected at once on cooling.

All stood at 20° C. in the dark.

In order to meet any such objection as that the original water possibly contained the spores of anthrax, we proceeded as follows :—

On June 27, the flasks having stood for seven days, we took samples of the original raw (non-infected) Thames water, and heated them at 60° C. for twenty-four hours. Plates made from this gave no signs of anthrax colonies. We also inoculated a guinea-pig with 1 c.c. of the raw water (not sterilised) injected into the peritoneum; this animal lived uninjured, whereas a guinea-pig inoculated with 1 c.c. of the raw water infected with anthrax died in thirty-six hours, and cover glass preparations and cultures made from the organs proved that it died normally of anthrax.

Table *a*.—Thames Water infected forthwith with *Virulent* Anthrax.

Date on which water was collected.	Date of making plate.	Number of days water stood at 20° C.	Number of days plate was incubated at 20° C.	Quantity of water used for plate. c.c.	Diluted or not.	Number of colonies on plate.		Calculated average number of bacteria per 1 c.c. of water.		Remarks.
						Total.	Anthrax.	Total.	Anthrax.	
June 20	June 20	0	8	$\frac{1}{4}$	*$\frac{1}{10}$th dilution	2,750	2744	137,500	137,200	Only 6 water colonies on the plate; all the rest=anthrax.
,,	,, 20	0	8	$\frac{1}{16}$	,,	7,200	7180	120,000	119,600	20 colonies on plate = water form.
,,	,, 21	1	8	$\frac{1}{4}$	$\frac{1}{10}$th dilution	600	12	60,000	1,200	
,,	,, 21	1	8	$\frac{1}{16}$	,,	740	7	74,000	700	A few very small and much retarded anthrax colonies.
,,	,, 22	2	3	$\frac{1}{4}$	,,	10,000	?	1,000,000	?	Probably over 1,000,000, but could neither count nor recognise anthrax; probably very few.
,,	,, 22	2	3	$\frac{1}{16}$	,,	Too many to count	?	?	?	We examined microscopically while the colonies were very small. Anthrax was present, but entirely swamped a few hours later by the water forms.
,,	,, 23	3	1	$\frac{1}{5}$	,,	5,000	Some	500,000	?	
,,	,, 24	4	2	$\frac{1}{4}$	,,	10,000	Some	1,000,000	?	A few anthrax colonies still there.
,,	,, 25	5	2	$\frac{1}{4}$	,,	..	..	..	..	Liquefied and spoilt.

* i.e., 1 : 9 of infecting fluid and sterile water respectively.

Table *a* shows very clearly how the anthrax rapidly falls in quantity during the first three days, whereas the normal aquatic flora takes the lead and runs through the usual phases of rapid rise to a maximum and then an eventual fall. I have not included their further behaviour here, however, because it was impossible to trace the anthrax any longer on the plates.

On June 27, we heated a sample of the water—having then stood seven days at 20° C.—at 60° C. for twenty-four hours, and made a series of plates from it. With the exception of one or two water organisms on one of the plates, we obtained beautifully pure cultures of anthrax, proving beyond doubt that spores had been introduced or formed in the flasks. This was confirmed by inoculating a guinea-pig.

Table *b*.—Thames Water filtered forthwith through Porcelain and infected with Virulent Anthrax.

Date on which water was collected.	Date of making plate.	Number of days water stood at 20° C.	Number of days plate was incubated at 20° C.	Quantity of water used for plate.	Diluted or not.	Number of colonies on plate.		Calculated average numbers of bacteria per 1 c.c. of water.		Remarks.
						Total.	Anthrax.	Total.	Anthrax.	
June 20	June 20	0	3	c.c. $\frac{1}{4}$	$\frac{1}{10}$th dilution	3000	8000	150,000	150,000	
,,	,, 20	0	3	$\frac{1}{4}$	,,	6000	6000	100,000	100,000	
,,	,, 21	1	3	$\frac{1}{4}$	$\frac{1}{10}$th dilution	1850	1350	135,000	185,000	
,,	,, 21	1	3	$\frac{1}{4}$	,,	2700	2700	90,000	90,000	
,,	,, 22	2	3	$\frac{1}{4}$	,,	1250	1250	125,000	125,000	
,,	,, 22	2	3	$\frac{1}{4}$	,,	1380	1380	46,000	46,000	
,,	,, 23	3	3	$\frac{1}{4}$	,,	1050	1050	105,000	105,000	
,,	,, 24	4	3	$\frac{1}{4}$	,,	1250	1250	125,000	125,000	
,,	,, 25	5	3	$\frac{1}{10}$	,,	750	750	75,000	75,000	

Table c.—Thames Water boiled for two hours and forthwith infected with *Virulent* Anthrax.

Date on which water was collected.	Date of making plate.	Number of days water stood at 20° C.	Number of days plate was incubated at 20° C.	Quantity of water used for plate.	Diluted or not.	Number of colonies on plates.		Calculated average numbers of bacteria per 1 c.c. of water.		Remarks.
						Total.	Anthrax.	Total.	Anthrax.	
June 20	June 20	0	8	c.c. ½	1/10th dilution	1650	1650	82,500	82,500	
,,	,, 20	0	8	½	,,	2100	2100	35,000	35,000	
,,	,, 21	1	3	½	1/50th dilution	280	280	28,000	28,000	Grew very slowly; we could not determine why.
,,	,, 21	1	3	3/5	,,	1650	1650	55,000	55,000	
,,	,, 22	2	3	½	,,	750	750	75,000	75,000	
,,	,, 22.	2	3	½	,,	900	900	30,000	30,000	Again a remarkable retardation of growth. We cannot explain it unless it is due to all the colonies arising now from *spores*.
,,	,, 23	8	8	½	,,	..	..	..	..	Culture spoilt and lost by an accident.
,,	,, 23	8	3	½	,,	1250	1250	48,000	48,000	
,,	,, 24	4	8	½	,,	660	660	66,000	66,000	
,,	,, 25	5	3	½	,,	1500	1500	150,000	150,000	

If we compare Table *a* with Tables *b* and *c*, there is a striking contrast as regards the maintenance of the anthrax colonies on the plates. This is no doubt largely due to the removal of the water organisms, enabling us to count the anthrax colonies so much more readily; but I do not believe it is solely due to that cause. It seemed much more likely—and a comparison of these tables with those of Series C appeared to bear out the probability—that the competition of the water organisms really affects the anthrax more directly, partly owing to the former taking what organic food materials there are, and so starving the anthrax, and partly owing to the rapid de-oxygenation of the water by the competing forms. As will be shown later, these normal water bacteria are aërobic in a very high degree, as we have convinced ourselves by actual experiments; and we have been surprised, therefore, at these results in the raw Thames water. As will be seen in the sequel, however, the behaviour of the organisms towards one another cannot be predicted (see pp. 290—298).

There is one point in connection with the *boiled* Thames water cultures (Table *c*) which seems worth further investigation: it is the remarkable retardation of growth exhibited on some of the plates after the first twenty-four hours. It seems by no means unlikely that the explanation is due to two causes :—

(1) The boiled water has been so far de-oxygenated that the *living* bacilli fall off, and only those which can pass into the spore condition maintain themselves, and as it takes longer to get cultures from the spores than from the actively vegetating bacilli, this might well explain the retardation seen on the plates.

(2) It may also be, however, that boiling the water renders many of the organic food substances less available for the growth of anthrax, and thus a partial starvation concurs in the fall.

Or (3) it may be due simply to osmotic phenomena consequent on immersion in the water.

In any case it seems worth while to note the apparently more rapid fall in the numbers in the *boiled* as contrasted with the *filtered* water, in the first forty-eight hours, though we think the matter would need a special inquiry to make certain of the phenomenon.

Series C.

This series of experiments was designed to secure answers to the following questions :—Do the bacilli of the anthrax live and multiply in Thames water at all, apart from any persistence of the spores ? If so, is there any difference in their behaviour in the crude water, taken fresh from the river, with all its normal bacteria flora and other impurities, and in the same water deprived of the aquatic microbes by filtration *through porcelain,* or sterilised by boiling? And, further,

T 2

does the behaviour of strong virulent anthrax, known to be capable of producing vigorous spores, differ from that of weak or "attenuated" anthrax, known to be less deadly to animals, though still capable of forming spores if the right conditions are offered, in any respects, and in any or all of the waters?

The virulent anthrax employed was obtained from Edinburgh (and recorded as Cow No. 3), and was proved to be fatal to rabbits in two days); the attenuated anthrax also came from Edinburgh (Cow No. 1), and took five days to kill a rabbit.

The experiments detailed in the following tables, C (I) to C (VI), were arranged as follows:—

I. Four flasks, two of a litre capacity, and two of half a litre each, were filled with the crude Thames water, brought fresh from the river, and infected forthwith with anthrax: one pair of flasks receiving strong anthrax, the other weak anthrax: plate cultures were made at once, and on each succeeding day, the flasks standing in an incubator at 20—22° C., the whole time.

II. Four similar flasks were filled with the Thames water, same collection, filtered forthwith through porcelain (Chamberland filter), and proved to be free from aquatic bacteria, and duplicate pairs treated in exactly the same way.

III. Four similar flasks were filled with the Thames water, same collection, and treated exactly as before, excepting that the water was sterilised by heating in a steam steriliser to 100° C. for two hours.

IV. Finally, four similar flasks were filled with the raw Thames water, exactly as in set I, excepting that no anthrax was added, as we wished to determine by daily plate culture how the water organisms of the normal water behaved apart from the anthrax.

As regards the incubation and future care, &c., all the 16 flasks were treated alike, and the conditions of comparison are, therefore, the same.

The infecting fluid was obtained as follows in each case:—Clean sowings were taken from an active agar culture, then shaken up with sterile distilled water, and some of the dilute sowing spread on potato (in tubes) and incubated for 24 hours at 30° C.

This gave vigorous vegetative cultures, free from spores, as we satisfied ourselves by placing samples at 60° C. for 18 hours, and then making plate cultures, and we then proceeded as follows:—

The potato cultures were broken down in sterile distilled water, care being taken to introduce as little potato as possible, and charges of this placed in the flasks. Of each pair of flasks infected, one received four times as much as the other; the charge is referred to in the tables as "large," or "small," accordingly. It may *here be stated that in those cases where experience showed us that large numbers of colonies were to be expected on the plates, we used*

not only small doses of the liquid to be tested, *e.g.*, 1 or 3 drops from a pipette discharging 25 drops to the 1 c.c., but also diluted the liquid with sterile distilled water, *e.g.*, 1 : 4, or 1 : 9, &c., facts which we bring out duly in the tables.

To those critics who would remark on the dangers of the "dilution method" above referred to, I would reply in two ways : (1) it is the only practicable method available for getting over the difficulty of plates so densely crowded with colonies that no attempt at counting (or estimating) is possible ; and (2) it certainly does not lead to *exaggeration* of the numbers of colonies, but in the contrary direction, and, therefore, the final numbers obtained are more likely to be below than above the truth.

As a matter of experience, I am, in fact, more and more assured that the whole procedure of gelatine plate cultivation leads to underestimation rather than to over-estimation, and this is obviously a fault on the better side of exactness, since we have to be content with approximations. Nevertheless, great care has to be taken in all stages of manipulation, and the more so because it is always necessary to multiply out the final results.

The results of the daily examination of the non-infected flasks show the usual rise to a maximum, and then fall in the numbers of normal aquatic organisms existing in the Thames water. I now pass to the results obtained with weak and strong anthrax respectively, in the crude Thames water, *i.e.*, without filtering or sterilising in any way, Tables C (I) and C (II).

SERIES C. Table c (I).—Crude Thames Water infected forthwith with *Strong Anthrax.*

Date on which water was collected.	Date of making plate.	Number of days water stood at 20° C.	Number of days plate was incubated at 20°C.	Charge of anthrax used.	Quantity of water used for plate.	Diluted or not.	Number of colonies on plate.		Calculated average numbers of bacteria per 1 c.c. of water.		Remarks.
					c.c.		Total.	Anthrax.	Total.	Anthrax.	
Mar. 22	Mar. 22	0	3	Large	1/1	Not	40	20	1000	500	The anthrax colonies were abundant, and about as many as the water forms, but liquefaction and crowding out made it impossible to count accurately.
"	" 22	0	3	"	1/10	"	Too many to count	Most were anthrax	?	?	23 colonies of organisms were found and innumerable anthrax. This would give 2300 water forms per 1 c.c., and anthrax in much higher proportion.
"	" 23	1	3	"	1/10	1/4th dilution	?	?	?	?*	Plate liquefied, but numerous anthrax colonies seen.
"	" 23	1	3	Small	1/50	"	?	?	144,000	?	Ditto.
"	" 24	2	2	"	1/50	"	288	12	200,000	6,000	
"	" 24	2	2	"	1/50	1/10th dilution	1200	800	133,300		
"	" 25	3	3	"	1/50		58	0	58,000	0	No anthrax visible on keeping another day.
"	" 25	3	3	"	1/10	"	180	?	43,300	?	No colonies of anthrax visible on this day, but next day we detected a few.
"	" 26	4	3	"	1/50	"	28	1	26,000	1,000	Here, again, a few feeble anthrax colonies were observable next day, evidently being crowded out.
"	" 26	4	3	"	1/50	"	80	?	23,300	?	
"	" 29	7	3	Large	1/50	"	180	35	32,500	8,750	Doubtful if any anthrax.
"	" 29	7	3	"	1/75	"	800	?	25,000	?	

* In order to be sure if anthrax was present a mouse was inoculated, with positive results.

SERIES C. Table *c* (II).—Crude Thames Water infected forthwith with *Weak Anthrax.*

Date on which water was collected.	Date of making plate.	Number of days water stood at 20° C.	Number of days plate was incubated at 20° C.	Charge of anthrax used.	Quantity of water used for plate.	Diluted or not.	Number of colonies on plate.		Calculated average number of bacteria per 1 c.c. of water.		Remarks.
					c.c.		Total.	Anthrax.	Total.	Anthrax.	
Mar. 22	Mar. 22	0	3	Large	$\frac{1}{1}$	Not	Innumerable	Innumerable	?	?	There were 4 water organisms on plate, which gives 100 per 1 c.c. and countless anthrax.
,,	,, 22	0	3	,,	$\frac{1}{1}$	,,	,,	,,	?	?	27 water colonies on plate = 225 per c.c. and innumerable anthrax.
,,	,, 23	1	1	,,	$\frac{1}{1}$	¼th dilution	?	?	?	?	Liquefied in 24 hours; anthrax was present, however.
,,	,, 23	1	1	Small	$\frac{1}{10}$	,,	?	?	?	?	,, ,, ,, *
,,	,, 24	2	2	,,	$\frac{1}{1}$	,,	508	11	204,000	5,500	
,,	,, 24	2	2	,,	$\frac{1}{10}$	,,	1160	?	193,300	?	Could not estimate the anthrax.
,,	,, 25	3	4	,,	$\frac{1}{1}$	⅒th dilution	180	8	130,000	3,000	The anthrax did not appear till 4th day; probably many suppressed.
,,	,, 25	3	4	,,	$\frac{1}{1}$	,,	221	35	73,600	11,600	
,,	,, 26	4	8	,,	$\frac{1}{10}$	,,	23	10	23,000	10,000	
,,	,, 26	4	8	,,	$\frac{1}{1}$	,,	85	25	28,300	8,300	
,,	,, 29	7	8	,,	$\frac{1}{10}$	,,	200	35	200,000	35,000	
,,	,, 29	7	8	,,	$\frac{1}{1}$	,,	350	Many	116,600	?	Could not estimate the anthrax, but very many colonies there.

* A mouse received 5 drops subcutaneously.

Series C. Table c (III).—Thames Water filtered through Porcelain, and forthwith infected with *Strong Anthrax*.

Date on which water was collected.	Date of making plate.	Number of days water stood at 20° C.	Number of days plate was incubated at 20° C.	Charge of anthrax used.	Quantity of water used for plate.	Diluted or not.	Number of colonies on plate.		Calculated average number of bacteria per 1 c.c. of water.		Remarks.
							Total.	Anthrax.	Total.	Anthrax.	
Mar. 22	Mar. 22	0	8	Large	c.c. 1/4	Not	4000 to 6000 ?	4000 to 6000 ?	100,000 to 150,000 ?	100,000 to 150,000 ?	Pure anthrax.
"	22	0	8	"	1/4	"					Too many to count; certainly not fewer than in last, and pure anthrax.
"	23	1	3	"	1/4	"	8000 to 5000 ?	8000 to 5000 ?	75,000 to 125,000 ?	75,000 to 125,000 ?	All anthrax.
"	24	3	5	Small	1/4	1/4th dilution	?	?	?	?	Many anthrax, and a few intruded forms; temperature had been allowed to fall, and development was too slow.
"	25	3	6	"	1/4	1/10th dilution	480 1040 ?	480 1040 ?	480,000 346,600 ?	480,000 346,600 ?	Very clean pure culture.
"	25	3	6	"	1/4	"					Too many to count, and a small intruder was present, still very numerous anthrax, and on the 3rd day the culture seemed pure anthrax.
"	29	7	6	Large	1/4	"					
"	28	7	8	"	1/4	"	Too many to count	Pure anthrax	Too many to estimate	Pure anthrax.	

Series C.

Table c (IV).—Thames Water filtered through Porcelain and forthwith infected with *Weak Anthrax*.

Date on which water was collected.	Date of making plates.	Number of days water stood at 20° C.	Number of days plate was incubated at 20° C.	Charge of anthrax used.	Quantity of water used for plate.	Diluted or not.	Number of colonies on plate.		Calculated average number of bacteria per 1 c.c. of water.		Remarks.
					c.c.		Total.	Anthrax.	Total.	Anthrax.	
Mar. 22	Mar. 22	0	8	Large	$\frac{1}{16}$	Not	600	600	15,000	15,000	
,,	,, 22	0	8	,,	$\frac{1}{16}$	,,	3500	3500	29,100	29,100	
,,	,, 23	1	4	,,	$\frac{1}{16}$	,,	2500	2500	62,500	62,500	
,,	,, 23	1	4	,,	$\frac{3}{16}$	,,	4500	4500	37,500	37,500	
,,	,, 24	2	8	Small	$\frac{1}{16}$	¼th dilution	850	?	425,000	?	A very large proportion was anthrax, but a foreign form had intruded into the flask.
,,	,, 24	8	8	,,	$\frac{3}{16}$	,,	Too many to count	All anthrax	?	?	
,,	,, 25	8	8	,,	$\frac{9}{16}$	$\frac{1}{16}$th dilution	?	?	?	?	Plate spoilt, but some anthrax was recognised.
,,	,, 29	7	6	Large	$\frac{1}{16}$	,,	4800	4800	1,200,000	1,200,000	
,,	,, 29	7	6	,,	$\frac{3}{16}$	,,	Too many to count	?	?	?	All pure anthrax, and certainly not fewer than last.

SERIES C.   Table c (V).—Thames Water sterilised forthwith by heat and infected with *Strong Anthrax*.

Date on which water was collected.	Date of making plate.	Number of days water stood at 20° C.	Number of days plate was incubated at 30° C.	Charge of anthrax used.	Quantity of water used for plate.	Diluted or not.	Number of colonies on plate.		Calculated average number of bacteria per 1 c.c. of water.		Remarks.
							Total.	Anthrax.	Total.	Anthrax.	
Mar. 22	Mar. 22	0	3	Large	c.c. 1/10	Not	5000	5000	125,000	125,000	Pure anthrax. Not fewer than in last case.
"	" 22	0	3	"	1/10	"	Too many to count	Too many to count	?	?	
"	" 23	1	3	"	1/25	"	5000 to 6000	5000 to 6000	125,000 to 150,000	125,000 to 150,000	As above; not fewer than before.
"	" 23	1	3	"	1/25	"	Too many to count	Too many to count	?	?	
"	" 24	2	5	Small	1/4	¼th dilution	1000	1000	500,000	500,000	One or two yellow colonies were not anthrax.
"	" 24	2	5	"	1/4	1/10th dilution	?	?	?	?	About same number as last.
"	" 25	3	6	"	1/4	"	2400	2400	2,400,000	2,400,000	
"	" 25	3	6	"	1/4	"	3600	3600	1,200,000	1,200,000	
"	" 29	7	6	Large	1/4	"	1200	1200	300,000	300,000	
"	" 29	7	6	"	1/4	"	1200	1200	100,000	100,000	Nothing visible on 4th day.

SERIES C.

Table c (VI).—Thames Water sterilised forthwith by heat and infected with *Weak Anthrax*.

Date on which water was collected.	Date of making plate.	Number of days water stood at 20° C.	Number of days plate was incubated.	Charge of anthrax used.	Quantity of water used for plate. c.c.	Diluted or not.	Number of colonies on plate.		Calculated average number of bacteria per 1 c.c. of water.		Remarks.
							Total.	Anthrax.	Total.	Anthrax.	
Mar. 22	Mar. 22	0	3	Large	1/1	Not	2500	2500	62,500	62,500	
"	" 22	0	3	"	1/4	"	Too many to count	?	75,000	?	Pure anthrax. Too numerous to estimate, but not fewer than last.
"	" 23	1	3	"	1/40	"	3000	3000	?	75,000	
"	" 23	1	3	"	1/40	"	Too many to count	?	275,000	?	As above.
"	" 24	2	3	Small	1/40	⅓th dilution	550	550	?	275,000	
"	" 24	2	3	"	1/40		2000	2000	333,300	333,300	
"	" 25	3	6	"	1/40	¹⁄₁₀th dilution	2000	2000	2,000,000	2,000,000	
"	" 25	3	6	"	1/40	"	4400	4400	1,466,600	1,466,600	
"	" 29	7	6	Large	1/40	"	2500	?	625,000	625,000	Not quite pure; a few yellow colonies had intruded.
"	" 29	7	6	"	1/40	"	Too many to count	?	?	?	Pure anthrax, but far too numerous to estimate.

Here we see, on examining Table C (I), in spite of unavoidable imperfections in the observations, due to the difficulties of counting and of observing when liquefaction commences, that both the anthrax and the water organisms may run a similar course as regards the first few days; in both cases the climax is rapidly reached (about the third day) and then a decline sets in. But it is worth notice that even after seven days' standing the anthrax is not eliminated, and we were so struck with the importance of this phenomenon that I decided to employ further tests to see if this persistence was really due to the continued vegetation of the anthrax or to the development of spores.

I was driven to suspect spores by several facts. In the first place Strauss and Dubarry have shown* that anthrax *can* form spores after being placed in water, provided the temperature is not too low (20° C.); secondly, we noted in some plates that the anthrax colonies were hanging back, so to speak, in their development, and it seemed not unlikely that this was due to time being needed for the germination of spores.

To test this point we placed a few cubic centimetres from one of the flasks of this group on April 7, *i.e.*, eight days after the last culture, for 24 hours at 60° C., and, before heating the liquid, inoculated a guinea-pig and a mouse with a trace of it.

Both guinea-pig and mouse were dead on April 9, *i.e.*, after 48 hours, and that their death was due to anthrax was proved by finding the bacilli in the blood of the heart, and by obtaining pure cultures therefrom.

The water heated to 60° C. for 24 hours gave pure cultures of anthrax also, showing conclusively that spores had been formed in the water. These cultures also justify the conclusion that aquatic normal forms did *not* develop spores, unless we assume that their spores are less resistent to moderately high temperatures. Without laying too much stress on the numbers, therefore, I think Table C (I) shows that while *Bacillus anthracis* can only live vegetatively and maintain its hold for about three days in the crude Thames water at 20° C., it can form spores there which enable it to live for a longer period,† and I conclude *not* that the competing water forms destroy the bacilli, but *that the decrease of anthrax on the plates is due partly to its passing into the spore condition, and to the*

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* See our First Report, p. 268.

† We shall show later on that these spores can remain alive for several months, a result well established by previous observers. Duclaux, for instance, found that there were spores still alive in some of Pasteur's old flasks which had been kept for twenty-one or twenty-two years, and showed that in those flasks where they had *died it was probably owing* to the acid or alkaline reaction of the media. (See *De Bary*, 'Lectures on Bacteria,' 1887, p. 54.)

aquatic forms developing so rapidly, and some of them so quickly liquefying the gelatine, that even when plenty of anthrax exists on the plates the latter are rendered useless before they can be got to develop visible colonies.

On comparing Tables C (III) and C (IV) the result comes out that both weak and strong anthrax can hold their own for some time in the filtered Thames water, and that this is not a mere case of their lying passive and unchanged in it; indeed, without laying undue stress on the actual numbers, the general result *seems* to be that this schizomycete multiplies vegetatively under the conditions given, and then passes over into spores. I say this *seems* to be the case; but it is much more likely that the apparent increase at first is due to *the breaking up of the bacilli into shorter rodlets, most of which die at last.*

That spores were really present we proved, as before, by submitting samples of each of the waters to 60° C. for 24 hours, and then cultivating plates from them; the beautifully pure cultures of anthrax obtained showed clearly that spores had been formed.

I are aware of the criticism that the vegetative growth exhibited by both the weak and strong anthrax was probably not entirely at the expense of the organic materials already in the filtered water, but was no doubt in part due to small quantities of food materials introduced with the infecting material (and possibly in part also due to substances derived from decomposing bacteria); but the reply is (1) that the quantity of food materials introduced by our mode of infection was very small, and (2) that it does not affect the practical question much, because in nature such minimum fouling of the water would be likely to occur when anthrax finds its way to the river. Of course the criticism should be borne in mind, however, and our experimental results do not support the idea that anthrax can multiply vegetatively in waters containing only minimum traces of food materials.

If we compare Tables c (V) and c (VI) the fact again appears to come out that the anthrax bacillus, whether strong or weak as regards its virulence, behaves very like an ordinary water form when first placed in Thames water sterilised by steam. Here, again, the explanation given above no doubt applies. Moreover, it is again evident that, as time goes on, the plates need more incubating to bring out the bacilli, and the numbers are then very large—*cf.* the events of the third day—a fact which again raised our suspicions as to the development of spores. As before, moreover, we tested this suspicion by heating a sample of each water to 60° C. for twenty-four hours, and obtained pure cultures of anthrax therefrom.

Series D.

This series of experiments was designed to seek answers to the following questions:—1. Can an anthrax known to be incapable of developing spores (asporogenous) maintain itself in the Thames water side by side with other organisms, or in the same deprived of the water forms by filtering through porcelain?[*]   2. Is there any appreciable difference in behaviour between the asporogenous anthrax and a virulent race known to be capable of developing spores ?

As matters turned out, we had to abandon the series in the middle, owing to the discovery that our so-called " asporogene " was a very much enfeebled form, but not utterly devoid of the sporogenous power,[†] and partly owing to a mishap with the filtered series. We select some of the results—see Tables D (I) to D (III)—because the approximate numbers obtained are useful; but we are engaged in repeating the series with a more reliable culture of the " asporogenous anthrax."

The arrangement of the flasks, &c., was much as before. Four flasks were filled with the crude Thames water, not infected, and examined daily. Four flasks were filled with the same collection of water, and to these anthrax was added as follows:—Two received virulent anthrax liquid in the proportion of 1 c.c. to every 25 c.c. of water; and two received the same proportion of liquid through which " asporogene " anthrax was distributed.

Finally four similar flasks were filled with the water (same batch), filtered forthwith through a Chamberland porcelain filter, and infected exactly as the last. The virulent anthrax came from an agar culture, growing well at 30° C. for 24 hours, and in excellent condition. The " asporogene " anthrax was also growing vigorously on agar under the same conditions. In each case the infecting liquid was made by evenly distributing quantities of the anthrax, as equal as possible in quantity and as free from agar as we could remove it from the tubes, in sterile distilled water. It will be noticed that all the plates were made with a dilution of 20 times as much water as corresponded to the original infected liquid. As before, the plates were made daily for a week, and the numbers given in the columns depend on several countings.

[*] We decided not to use the water sterilised by heat in this series, in order to reduce the number of flasks and plates which it would entail.

[†] The asporogenous anthrax employed was not the spontaneous natural form, but one produced artificially by degeneration with carbolic acid.

SERIES D.

Table D (I).—Crude Thames Water infected forthwith with *Virulent Anthrax*.

Date on which water was collected.	Date of making plate.	Number of days water stood at 20° C.	Number of days plate was incubated at 20° C.	Quantity of water used for plate.	Diluted or not.	Number of colonies on plate. Total.	Anthrax.	Calculated average number of bacteria per 1 c.c. of water. Total.	Anthrax.	Remarks.
May 12	May 12	0	7	¼ c.c.	1/10th dilution	4	0	400	0	No anthrax appeared.
,,	,, 12	0	7	½	,,	792	780	26,400	26,000	
,,	,, 13	1	8	¼	,,	2200	440	220,000	44,000	
,,	,, 13	1	8	¼	,,	4000	?	183,300	?	The majority, by far, were anthrax, but the separate estimate could not be made, as the plate was liquefying rapidly.
,,	,, 14	2	8	¼	,,	9600	320	960,000	32,000	
,,	,, 14	2	8	½	,,	?	?	?	?	Uncountable, owing to the rapid liquefaction of plate.
,,	,, 16	4	8	¼	,,	2800	?	280,000	?	Chiefly, but not entirely, water forms.
,,	,, 16	4	8	¼	,,	2880	0	144,000	0	No anthrax visible.

SERIES D.

Table D (II).—Crude Thames Water infected forthwith with *Asporogene* Anthrax.

Date on which water was collected.	Date of making plate.	Number of days water stood at 20° C.	Number of days plate was incubated at 20° C.	Quantity of water used for plate.	Diluted or not.	Number of colonies on plates.		Calculated average number of bacteria per 1 c.c. of water.		Remarks.
						Total.	Anthrax.	Total.	Anthrax.	
May 12	May 12	0	7	c.c. $\frac{1}{16}$	$\frac{1}{100}$th dilution	Many	Many	?	?	9 water forms appeared (=900 per c.c.), and towards the fifth day very numerous anthrax.
„	„ 12	0	7	$\frac{1}{16}$	„	„	„	?	?	Here also 32 water forms soon appeared (=1066 per c.c.), but, though very numerous anthrax appeared towards fifth day, the plate was already liquefying badly.
„	„ 13	1	5	$\frac{1}{16}$	„	2,000	1000	200,000	100,000	Plate liquefied, but about the same numbers.
„	„ 13	1	5	$\frac{1}{160}$	„	„	„	„	„	
„	„ 14	2	2	$\frac{1}{16}$	„	18,680	0	1,868,000	0	Possibly anthrax not yet developed; a few appeared later, but could not estimate.
„	„ 16	4	3	$\frac{1}{16}$	„	2,800	0	280,000	0	The anthrax seemed to have entirely disappeared.
„	„ 16	4	8		„	2,080	0	104,000	0	„ „ „
„	„ 18	6	3		„	580	0	58,000	0	„ „ „
„	„ 18	6	3		„	1,345	0	67,250	0	„ „ „

Series D.

Table D (III).—Thames Water filtered forthwith through Porcelain, and infected with *Virulent Anthrax.*

Date on which water was collected.	Date of making plate.	Number of days water stood at 20° C.	Number of days plate was incubated at 20° C.	Quantity of water used for plate. c.c.	Diluted or not.	Number of colonies on plate.		Calculated average number of bacteria per 1 c.c of water.		Remarks.
						Total.	Anthrax.	Total.	Anthrax.	
May 2	May 12	0	8	$\frac{1}{4}$	$\frac{1}{10}$th dilution	224	200	22,400	20,000	The few other forms were a yellow intruder. Not fewer than last.
,,	,, 12	0	3	$\frac{1}{4}$	,,	Too many to count	All anthrax	?	?	
,,	,, 13	1	5	$\frac{1}{4}$	,,	80	80	8,000	8,000	On the third day the colonies were so small that we could not count them: the (pure) cultures were, therefore, kept longer.
,,	,, 13	1	5	$\frac{1}{4}$	,,	360	360	12,000	12,000	
,,	,, 14	2	4	$\frac{1}{4}$	,,	200	200	20,000	20,000	
,,	,, 14	2	4	$\frac{1}{4}$	,,	360	360	36,000	36,000	
,,	,, 16	4	4	$\frac{1}{4}$	,,	280	220	22,000	22,000	
,,	,, 16	4	4	$\frac{1}{4}$	,,	...	...	...	...	Plate spoilt.
,,	,, 18	6	4	$\frac{1}{4}$	,,	200	200	20,000	20,000	

On comparing the Tables D (I) and D (II), we see that the water forms pass through the usual stages of rapid rise in numbers during the first two or three days, followed by a relapse to fewer and fewer; but it is suggestive that the enfeebled "asporogene" anthrax seemed to disappear more rapidly from the plates—Table D (II). This rapid disappearance did not occur where the water was sterilised by filtration, however, as Table D (III) shows, and I was for some time inclined to attribute this to the influence of the normal aquatic bacteria. It may still turn out to be so, but my experiments on p. 290 do not support the idea, and our confidence being shaken in the character of the "asporogene" anthrax used, I do not press the point, but hope to raise the question in another form at a later date.

The evidence goes to show, then, that *Bacillus anthracis*, while *only capable of living for a short time in the Thames water in the vegetative state, is able to persist very much longer in the form of spores*.

Bacteriological Examination of some Old Culture Flasks.

On October 6, it was decided to analyse the contents of five flasks, selected from a series which had been put aside for this purpose on the 5th March previous. The analysis of these five flasks is very interesting.

The flasks were labelled A, B, B', C, and D, and had been treated in various ways, as described below.

A was a litre flask, which on March 5 had been charged with about 600 c.c. of fresh Thames water, inoculated with a comparatively large dose (10 c.c.) of the same water, in which a vigorous potato culture of anthrax had been shaken up. The culture used was known to contain spores, and a good deal of starch had also been carried over.

The flask stood during the whole of the period (spring and summer) under a glass bell-jar, on a table near a north window, and received no direct sunlight, but ordinary bright daylight every day.

It was occasionally opened (the cotton-wool plug being carefully and quickly removed and replaced each time), to remove samples for analysis.

The temperature rarely fell below 12° C., or rose beyond 15° C., and a slight growth of green microscopic algæ made its appearance during the summer.

On October 6 cultures were made (1) direct from the flask (2) from same after exposure to 56° C. for 24 hours.

The direct cultures on gelatine gave plenty of bacteria, &c., but if any anthrax was there it was overwhelmed by the alien forms, or would not grow.

The cultures, after exposure at 56° C., gave no results on gelatine, *but animals inoculated with 2 c.c. died in five days of anthrax.*

The results (see Table I') showed that spores had remained alive for seven months in the flask, and though enfeebled they were capable of germinating and killing.

Flask B, *March 5 to October* 6, 1892.—B was also a 1-litre flask, filled with about 500 c.c. Thames water, and inoculated with 8 c.c. of the anthrax infection.

Kept in the open laboratory. Cotton-wool plug. Under bell-jar. At east window. Partial insolation in mornings. Temperature varied from 12°to 20° C.

After being thus undisturbed till October 6—*i.e.*, seven months— the plates showed no anthrax, though numerous other (water) bacteria were present.

A guinea pig inoculated with 2 c.c. of the water did not die in fourteen days.

Cultures from plates of the water, after exposure at 56° for twenty-four hours, showed no anthrax.

This result shows either (1) insolation killed off the spores, or (2) the temperature was not high enough for spores to form. That the first suggestion is the right one will be shown in the sequel.

Flask B', *March* 5 *to October* 6, 1892.—1-litre flask of Thames water exactly like B, and inoculated in the same way.

The only difference in treatment was that B' was placed at 20° C. in the incubator, and remained two months at that temperature; then, still in incubator, it was left through the summer in the dark, at the same temperature as B.

Plates on October 6 (*i.e.*, seven months later) gave no anthrax, either from water direct or after twenty-four hours at 56° C. Any anthrax-like colonies turned out to be saprophytes.

Nevertheless a guinea pig inoculated with 2 c.c. of the water died in three days, and cultures of anthrax were made from the heart.

Suggests that spores developed well, and kept well, but that they were either too few or too feeble to be easily detected on gelatine.

Flask C, *March* 5 *to October* 6, 1892.—1-litre flask filled with Thames water and boiled for half an hour, and then inoculated with 2 c.c. (therefore water organisms introduced), exactly as before.

Kept at 20° C. for two months in dark incubator, then remained (in same) for five months at ordinary temperatures.

On October 6 plate cultures gave no anthrax, though other organisms were shown. After twenty-four hours at 56° C., plates also gave no results; but a guinea pig inoculated with 2 c.c. of the water died in five days, and the heart's blood gave cultures of anthrax.

Suggests that boiling the Thames water in no way hurts it as a medium for anthrax to sporify in.

Flask D, *March* 5 *to October* 6, 1892.—1-litre flask filled with crude Thames water, and not inoculated.

υ 2

Kept in incubator at 20° C. for two months, then (same place) at ordinary temperature for five months more.

Plates gave no anthrax, either raw or after the water was at 56° C. for twenty-four hours, yet other organisms were found.

A guinea pig inoculated with 2 c.c. of the water lived.

The experiment suggests that there was no anthrax in the original Thames water, or it would have made itself evident during the sojourn in the incubator.

These results are summarised in the following table :—

Table F.—Examination of Flasks A, B, B¹, C, and D of March 5 to October 6.

Flask.	Contents of flask.	Date on which water was collected.	Date of making plates.	Number of days the water had stood.	Exposed to light or not.	Temperature at which water stood. °C.	Time plate was incubated. days.	Quantity of water used for plates. c.c.	Diluted or not.	Number of colonies on plates. Total.	Anthrax.	Remarks.
A	raw water infected	Mar. 5	Oct. 6	216	ordinary daylight	10—15	5	$\frac{1}{30}$	$\frac{1}{10}$th dilution	1	0	Corresponds to 300 bacteria per 1 c.c.
,,	,,	,,	,,	,,	,,	,,	8	,,	,,	7	0	One of the colonies, suspiciously like anthrax when young, was isolated and proved not anthrax.
,,	,,	,,	,,	,,	,,	,,	5	$\frac{1}{30}$	,,	8	0	Corresponds to 225 bacteria per 1 c.c.
,,	,,	,,	,,	,,	,, east window	12—20	8	,,	,,	15	0	Three colonies very like typhoid in appearance, but isolated and proved different.
B	,,	,,	,,	,,	window		5	$\frac{1}{30}$	,,	85	0	
,,	,,	,,	,,	,,	,,	,,	8	,,	,,	22	0	
,,	,,	,,	,,	,,	,,	,,	5	$\frac{1}{30}$	,,	90	0	One anthrax-like colony proved to be not anthrax.
,,	,,	,,	,,	,,	,,	,,	8	,,	,,	100	0	
B¹	,,	,,	,,	,,	in dark	20*	5	$\frac{1}{30}$	,,	40	0	Nine liquefying.
,,	,,	,,	,,	,,	,,	,,	8	,,	,,	26	0	
,,	,,	,,	,,	,,	,,	,,	5	$\frac{1}{30}$	,,	200	0	
,,	,,	,,	,,	,,	,,	,,	8	,,	,,	115	0	Several badly liquefying.

* In this case the flask was at ordinary summer temperature, 14—20° C., after August 1, but in dark incubator.

Table F—*continued*.

Flask.	Contents of flask.	Date on which water was collected.	Date of making plates.	Number of days the water had stood.	Exposed to light or not.	Temperature at which water stood.	Time plate was incubated.	Quantity of water used for plates.	Diluted or not.	Number of colonies on plates — Total.	Number of colonies on plates — Anthrax.	Remarks.
C	boiled water infected	Mar. 5	Oct. 6	216	ordinary daylight	°C. 10—15	days. 5	c.c. $\frac{1}{10}$	$\frac{1}{10}$th dilution	6	0	It is interesting to note how much fewer the water colonies were in the flask of *boiled* water.
,,	,,	,,	,,	,,	,,	,,	8	,,	,,	9	0	
,,	,,	,,	,,	,,	,,	,,	5	$\frac{4}{10}$	,,	10	0	
,,	,,	,,	,,	,,	,,	,,	8	,,	,,	24	0	
D	raw water not infected	,,	,,	,,	,,	,,	5	$\frac{1}{10}$	,,	4	0	
,,	,,	,,	,,	,,	,,	,,	8	,,	,,	10	0	
,,	,,	,,	,,	,,	,,	,,	5	$\frac{4}{10}$	,,	40	0	
,,	,,	,,	,,	,,	,,	,,	8	,,	,,	48	0	

On October 26, 1892, we examined the contents of a flask which had stood since March 5, 1892, in a north window of the laboratory, *i.e.*, it had remained nearly eight months undisturbed, at ordinary temperatures and in diffused daylight.

This flask was an interesting one in many respects. When first placed in position, on March 5, it had received a charge of about 300 c.c. of fresh Thames water, infected with a very strong charge of virulent anthrax taken from a potato culture, but we discarded it at the time because (1) the culture was found to contain so many spores, and (2) so much of the starch of the potato had been transmitted with the charge that we judged it better to renew the experiments we were engaged in.

During the summer the water in this flask became quite green with microscopic algæ, evidently developed from the Thames water, and it seemed worth while on October 26 to test the water for anthrax, to see if the presence and activity of the algæ had eliminated that organism.

The positive results of the analysis are shown below in Table G. On several of the plates no anthrax could be found at all; but in the two cases recorded in the table there was no doubt whatever.

Table G.—Bacteriological Analysis of Stock Flask of Thames Water infected with Anthrax and green with Algæ after standing at ordinary temperatures from March 5 to October 26 in diffused light.

Date of making plate.	Quantity used for plate.	Diluted or not.	Number of colonies on plate.		Number of bacteria per 1 c.c. of original.		Remarks.
			Total.	Anthrax.	Total.	Anthrax.	
26.10.92	c.c. $\frac{1}{10}$	$\frac{1}{11}$th dilution	200 to 800	1	66,000 to 99,000	330	Only one *undoubted* anthrax colony could be found. Total absence of liquefying bacteria noteworthy.
	$\frac{8}{10}$	,,	800 to 12,000	1	44,000 to 66,000	55	Again, only one anthrax colony could be discovered with certainty.

Two other facts are worth note in these analyses. (1) The large number of water organisms which had persisted through the seven to eight months, and (2) the total absence of rapidly liquefying forms.

It was already clearly proved then that the presence of the green algæ and the diffuse daylight had not exterminated the anthrax, although the numbers were extremely diminished, for on starting the experiment our flask contained something like 1,000,000 per 1 c.c., and more.

To place the matter still further beyond doubt, however, we placed 25 c.c. of the water at 56° C. for twenty-four hours, and obtained 1 anthrax colony in a 3-drop plate, and several plates with no trace of anthrax; then we got a 3-drop plate, among several with negative results, showing 5 anthrax colonies, and finally a 12-drop plate with 38 anthrax colonies.

It was observed that in all these cultures the anthrax colonies came on very slowly, and had there been any liquefying bacteria present, it is practically certain that we should have missed the anthrax altogether.

The conclusion is inevitable that although the anthrax had not been eliminated from this flask, it had been enormously diminished in quantity, and enfeebled as regards the powers of germination of the spores.

This conclusion was made a certainty by the following test:—On November 7 a guinea-pig was inoculated intra-peritoneally with 2½ c.c.* of the water in the flask; at first we thought it had escaped, but it died on November 17. Cultures from the heart's blood proved that it had died of anthrax; but it took ten days for the feeble and few spores to do the work.

The chief interest attaching to this series of experiments, however, is the proof that *insolation rapidly rids the water of the spores of anthrax.* I shall show later on that this is not only a very definite action, but one capable of being more directly and easily demonstrated than has hitherto been suspected.†

Experimental Observations on the Bacterial Flora of the Thames.

As already pointed out (p. 244), I have devoted considerable attention to the normal aquatic bacteria of the river water, isolating each form for further culture as it turned up in the course of the

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* This large dose was given because we found so few spores, and these apparently very much enfeebled.

† See our First Report ('Roy. Soc. Proc.,' vol. 51, 1892), pp. 199 and 237, for the literature dealing with the action of light on bacteria. See also pp. 308 and 310 of this Report.

investigation. Indeed, some of the first questions I set myself were the following:—(1.) What Schizomycetes are ordinarily found in the water? (2.) Are any of them pathogenic? (3.) Does *Bacillus anthracis* occur in the Thames? And (4) how do the aquatic forms behave in cultures?

Our preliminary examinations showed that several forms of Schizomycetes can be distinguished as common in the water, while here and there a yeast and a mould have been met with. I shall defer the consideration of all the other forms, with the exception of the one treated below.

Of these several forms we have been strongly impressed with the characters of some, while others have shown such slight individuality that it is difficult to be sure of their autonomy.

Of the well marked forms, one very common one is particularly characterised by its rapid growth and liquefaction of the gelatine, with a greenish hue and slightly putrid odour. This form is very like one of the forms known as *Bacterium termo*, and separated by Macé as *Bacillus termo*.[*] For some time we thought this was the species referred to, but prolonged and careful isolations and cultures have shown quite clearly that it is the form described by Flügge under the very descriptive name of *Bacillus fluorescens liquefaciens*.[†]

It occurs on the gelatine plates at all ordinary temperatures up to 20° C., as minute, greyish-white points, which rapidly enlarge to circles, and soon begin to liquefy, so that the colony lies at the base of a perfectly circular concave depression as a granular flocculent mass with a tinge of green, and with irregular radiations or networks into the liquefied circular area.

In from twenty-four to forty-eight hours the area of liquefaction extends very rapidly—in forty-eight hours at 12—15° C. the colonies were each as big as a shilling—and soon floods the plate with a slightly malodorous slimy fluid, of a pale emerald-green hue, with a very distinct fluorescent shimmer.

Such colonies consist of very short fine rods, often with a slight constriction, or in couples, and then difficult to distinguish from chains of cocci, from 2 to 3·5 μ long by 0·5 to 0·8 μ broad (measured after staining in Spiller's purple and mounting in Canada balsam); in no case have we found filaments or spores in the gelatine cultures.

If transferred to gelatine tubes, the same depression, rapid·liquefaction, and green fluorescence are observed at 12°, 15°, and 20° C.; and if the culture is made as a "stab," a very characteristic series of events follow. The liquefaction of the gelatine proceeds so as to form a funnel, very like a "thistle-head" in shape, and the flocculent greyish-white colonies fall slowly to the bottom of the rapidly

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[*] Macé, 'Traité pratique de Bactériologie,' pp. 585—587.
[†] Flügge, 'Die Mikroorganismen.'

widening "stem," which is filled with the liquid. The peculiar green fluorescence is very marked in these cases, masking somewhat the true colour of the colony itself.

On agar the colonies rapidly spread at 20—25° C. (more slowly at 12—15° C.), as a thin, wet-looking, or almost waxy, greenish-white layer, becoming thicker eventually, and very smooth and glassy, and tinging the subjacent agar with the characteristic hue. Here and there longer rods can be found in agar cultures, but no trace of spore formation could be discovered either on this or any other medium.

On potato the colonies are brownish-yellow, becoming deeper with age, and often with a raised, rough, granular surface, moist or oily in appearance. On all the solid media the fact that this bacillus is strictly aërobic comes out very strongly. Its growth is at once inhibited if a sterilised glass cover slip is placed over the young colony : the liquid rapidly fills up all the interspaces, and no air can enter, and growth stops at once. Moreover, if a " stab " culture is carefully made and covered with gelatine, the same inhibition is noticed.

The marked and rapid disappearance of this form from water which is kept standing in a closed vessel is almost certainly to be attributed to the same cause.

We have made numerous attempts to cultivate this form in hanging drops of gelatine, and with success, but there are no special points to notice : the rods divide very rapidly, and never grow out into long filaments or form spores in the moist chambers. It is impossible to cultivate it under a cover slip in compressed gelatine.

Milk is rendered slightly acid by the bacillus, and coagulation and peptonisation follow.

The slimy, green, fluorescent liquid presents several interesting features. Slight quantities of acid—hydrochloric or acetic—cause the green colour to disappear; but neutralisation with ammonia at once restores it, and an excess of the alkali deepens the hue. The colour is not destroyed by boiling, though, if prolonged, the green hue becomes distinctly paler. This agrees exactly with Macé's account of it, and is no doubt strong confirmatory evidence as to the correctness of our identification of the Schizomycete.

The annexed table summarises the character of this Schizomycete.

Characteristics of *Bacillus fluorescens liquefaciens* (Flügge).

Habitat.....	Thames water at all seasons.
Morphological characters	Very small short rodlets, average size 1—1·5 μ × 0·5 μ, sometimes in pairs or short chains, and often slightly constricted.
Spores......	None found in any medium.
Colonies on gelatine plates	Small, circular, rapidly enlarging to the size of a shilling and more, and then forming liquefied depressions with perfectly circular clear edges, and flocculent greenish-white granular masses of the organism floating in the centre, often with networks radiating from denser centre.
In gelatine tubes	*Streak cultures* rapidly liquefying at all ordinary temperatures, and the liquefied slimy gelatine with a yellowish-emeral hue, fluorescing greenish-blue. Slight putrefactive odour. *Stab cultures* very characteristic. The liquefaction begins above, forming a concave funnel-like depression, and extends down the puncture; in two or three days a "thistle-head" funnel of liquefied gelatine, very green and fluorescent, especially above. The greyish flocculent colonies gradually settle down the stem of the "funnel," widening the area of liquefaction above.
On agar	At all ordinary temperatures to 20—25° C., spreading as a greenish-white wet layer, at first thin. The fluorescent green tinge penetrates a couple of millimetres or more into the agar, which remains solid.
On potato...	In five days at 20—25° C. forms a yellowish-brown, and shining, often granular layer, deepening in colour subsequently.
In bouillon..	Turbidity, green fluorescence, and deposit.
In milk.....	Precipitates the casein, and then completely peptonises it. Acid reaction. Liquid clear in 10—14 days at ordinary temperatures.
Temperature	Grows well at all temperatures from 10° C. to 25° C. Cardinal points not determined.
Rapidity....	Very rapid development and liquefaction. It is the earliest form to appear on gelatine plates.
Air requirements	Markedly aërobian. Will not grow under glass plates, or submerged in solid media.
Light.......	Grows well in the dark.
Pigment	Soluble in the medium. Becomes paler on boiling, but is not destroyed. Disappears at once if acidified with hydrochloric or acetic acid, but reappears on neutralising with ammonia; an excess of alkali deepens the green fluorescent hue.
Pathogenic *or not*	Not. It is a chromogenic saprophyte, and grows well in water containing mere traces of soluble organic materials.

If we now attempt to discover this form among the species recorded in Eisenberg, Macé, and Roux, there appear to be only the following to choose from as the known liquefying forms which produce the characteristic green fluorescence in gelatine, or which possess any green pigment at all:*—*Bacillus fluorescens liquefaciens* (Flügge), *B. fluorescens liquefaciens minutissimus* (Unna), *B. fluorescens nivalis* (Schmolck), *B. viscosus* (Frankland), and *B. termo* (Duj. and Macé). We will also notice *B. aërophilus* (Libor.) and *B. chlorinus* (Engelm.) as being accompanied by a green colour in the cultures. We may at once eliminate *B. chlorinus*, however, because, according to Engelmann's description (he called it *Bacterium chlorinum*), the green colouring matter is in the cells, and these are much too large for our form. The same applies to Van Tieghem's *Bacillus viridis* and *B. virens*, and we are not concerned here with the discussion as to the chlorophyll nature of the colour or the claims of these forms to be regarded as Schizomycetes at all.

B. aërophilus may also be readily eliminated, for, apart from the large size of its filaments, it grows very slowly, and does not colour the gelatine; its green colour is confined to the colonies.

B. fluorescens liquefaciens (Flügge) presents startling points of similarity with our form.

It agrees in habitat, water and air, &c., as well as in the mean size and union of the rodlets, and is motile.

The colonies on gelatine plates are, like our form, round and depressed in funnel form, and the very regular clear zone of liquefaction is characteristic, as also the mode of liquefaction and fluorescence.

On agar, too, according to Macé, the growths are quite similar, while the very marked aërobian character and rapid growth are also alike.

In short, we find no important differences between the descriptions, and therefore regard this form as identical with Flügge's *B. fluorescens liquefaciens*.

B. fluorescens liquefaciens minutissimus (Unna), whether a good species or not, presents sufficient differences to separate it from ours. Apart from its habitat, the feeble fluorescence and capacity for growing anaërobically would seem to separate it.

B. fluorescens nivalis (Schmolck) is a form found in the glacier waters of Norway, and reminds one forcibly of *B. fluorescens liquefaciens*. The information to hand is too meagre to enable us to decide.

B. viscosus (Frankland) is possibly eliminated by the characters of the colonies, if the author's description of the radiating hair-like marginal growths is characteristic. Nor does the size quite agree, though the discrepancies could hardly be insisted upon. In other

* See our *First Report*, Appendix B, for literature concerning these forms.

respects, however (e.g., as regards habitat and mode of liquefaction, &c.), there are suggestive resemblances, and Macé regards it as identical with *B. fluorescens liquefaciens*.

Bacillus termo is the name given by Macé to a form which he separated from the mixture which previously passed under the name of *Bacterium termo* (Dujardin), and it presents several impressive similarities to our form.

It is very common in water, and has the size and shape and modes of union and movement observed. In fact almost the only serious discrepancy we find as regards the habit of the two forms is that ours has not the peculiar wavy or "amoeboid" contour of Macé's form.

As will readily be understood, the critical examination of these water bacteria is a matter of time, and requires much care; nevertheless, I regard it as of great importance to the object we have in view, and propose now to give an example of one of the lines of inquiry which ought to be pursued as closely as possible, and which can only be properly pursued when the separate water organisms have been thoroughly studied.

Experiments on the Behaviour of Anthrax sown simultaneously with B. fluorescens liquefaciens in Water.

For many reasons, and especially because this is a common water form, perhaps the commonest Schizomycete in the Thames, it seemed worth while to try the effect of sowing *Bacillus anthracis* and this *B. fluorescens liquefaciens* together in the same water. The following experiments were accordingly carried out :—

A litre flask was filled about three-quarters full (700 c.c.) of sterilised distilled water and plugged with cotton wool, and the whole carefully sterilised and allowed to cool, and a very dense sowing of a mixture of active anthrax and of *B. fluorescens* put into the water. The sowing was accomplished as follows :—10 platinum loops full of a vigorous two-day agar culture of anthrax were rubbed into a sterile test-tube with 1 c.c. of sterilised distilled water; then 10 loops of a *B. fluorescens* culture as equal as possible were treated in the same way in another tube, and the contents of the two tubes mixed and shaken thoroughly. 1 c.c. of this mixture was finally put into the 700 c.c. of the sterilised distilled water and thoroughly mixed by shaking. The flask was then placed in the dark incubator at 20° C. Every effort was made to introduce as little of the culture media (agar) as possible, and to sow approximately equal quantities of each Schizomycete, though of course it was impossible to attain these objects completely. Then plate cultures were made day after day— starting with one made directly on sowing the bacteria—to see what, if any, effect the struggle for existence would present.

The results are shown in the annexed table :—

Table z.—*B. anthracis* and *B. fluorescens liquefaciens* together in Distilled Water, at 20° C., in the dark.

Number of days flask stood.	Date of making plate.	Number of days plate was incubated at 20° C.	Quantity of water used for plate. (c.c.)	Diluted or not.	Number of colonies found on plate.		Calculated total number of bacteria per 1 c.c. original.		Remarks.
					Anthrax.	B. fluorescens.	Anthrax.	B. fluorescens.	
0	Nov. 16	3	$\frac{1}{16}$	Diluted 49 : 1	8	2	8750	2500	
1	,, 17	3	$\frac{1}{16}$	,,	3	4	3750	5000	
2	,, 18	3	$\frac{1}{16}$	,,	2	1	2500	1250	
3	,, 19	3	$\frac{1}{16}$	,,	1	0*	1250	0	*1 *Mould.*
5	,, 21	4	$\frac{1}{16}$	24 : 1	2	2	1250	1250	Badly liquefied.
6	,, 22	3	$\frac{1}{8}$	,,	28†	13	3500	1625	† A little doubt as to *all* being anthrax?
6	,, 22	3	$\frac{1}{8}$	,,	15	18	1875	2250	
9	,, 25	3	$\frac{1}{16}$	29 : 1	1	0‡	750	0	‡1 *Sarcina* (intruded?).
9	,, 25	3	$\frac{1}{16}$	,,	0	0	0	0	
10	,, 26	3	$\frac{1}{16}$	,,	0§	0	0	0	§1 *mould* (intruded?).
13	,, 29	3	$\frac{1}{16}$	Not diluted	17	0‖	425	0	‖1 *mould* and 2 colonies of an intruded form.
15	Dec. 1	3	$\frac{1}{16}$	,,	2	0	50	0	
15	,, 1	3	$\frac{1}{16}$	,,	0	0	0	0	
18	,, 4	5	$\frac{1}{4}$	,,	2	0	8	0	These four plates were made by pouring a large tube of gelatine infected with 1 c.c of the water, into four plates. Average ∴ = 34 per 1 c.c.
18	,, 4	5	$\frac{1}{4}$	,,	24	0	96	0	
18	,, 4	5	$\frac{1}{4}$	,,	0	0	0	0	
18	,, 4	5	$\frac{1}{4}$	,,	8	0	32	0	
28	,, 14	5	$\frac{1}{4}$	,,	7	0	35	0	Contents of a 10-drop tube on two plates. Average = 30 per c.c.
28	,, 14	5	$\frac{1}{4}$	,,	5	0	25	0	
37	,, 23	5	$\frac{1}{4}$	,,	2	0	5	0	
42	,, 28	.	$\frac{1}{4}$	,,	33	0	82	0	

On carefully going over this table, which illustrates very clearly the results of the struggle for existence between the two organisms, it is evident (1) that both the anthrax and the *Bacillus fluorescens liquefaciens* fall off in numbers day after day—for it cannot be urged that the doubtful high number found on the sixth day invalidates the general results—and (2) that the anthrax persists simply because it passes into the spore condition.

To make this last point quite certain, we heated a few c.c. of the water to 54° C. for twenty-four hours, and then made new plates, with the expected result: there were anthrax spores present.

Possibly the third inference to be drawn from this experiment is the most interesting of all, viz., that *anthrax can hold its own in distilled water at* 20° *O., in the dark, not only in spite of the presence of* B. fluorescens, *but* (*owing to its power of forming spores*) *it may even tire out and exterminate the latter, because it cannot form spores in the water.*

These results show with startling· clearness how cautious we must be in forming any opinions about the mutual relations between any two bacteria, or, indeed, between any two organisms whatever. The whole subject is a matter of biological experiment, and cannot be predicted on any other grounds than those of actual experiment. It is very commonly supposed that a saprophyte like *B. fluorescens liquefaciens* must be able to drive out a parasite like *B. anthracis* if the latter invades the territory of the former; the above experiment shows that such is by no means the case under the conditions afforded, and we ought to be very cautious indeed in surmising what will occur under other conditions of the same general kind..

In view of the facts above recorded, it seemed advisable to test the question in another way, and we did this as follows :—

A sterilised glass cylinder, *A*, receives a porcelain filter, *B*, also sterilised, and held in position and protected from dust, &c., by a covering and plug of sterilised cotton wool, *C*, and a paper cap. After final sterilisation, the cylinders received about 300 c.c. of Thames water, sterilised by filtration through porcelain. *Bacillus fluorescens liquefaciens* was sown in the water *inside* the filter *B*, and *anthrax* in the water *outside* the porcelain. Since experiments showed that the green fluorescing liquid of the former passes readily through such a filter, and that the excretions made by anthrax do the same, it was hoped that this apparatus would help to answer the question, Do either of these bacilli poison the water for the other ?

The results, after twenty-four days' incubation at 20° C. in the dark, proved that both the organisms were still alive, the anthrax being chiefly or entirely in the form of. spores. On heating the whole apparatus up to 56° C. for twelve hours, the plates showed *total absence of* B. *liquefaciens fluorescens*—proving that it developed
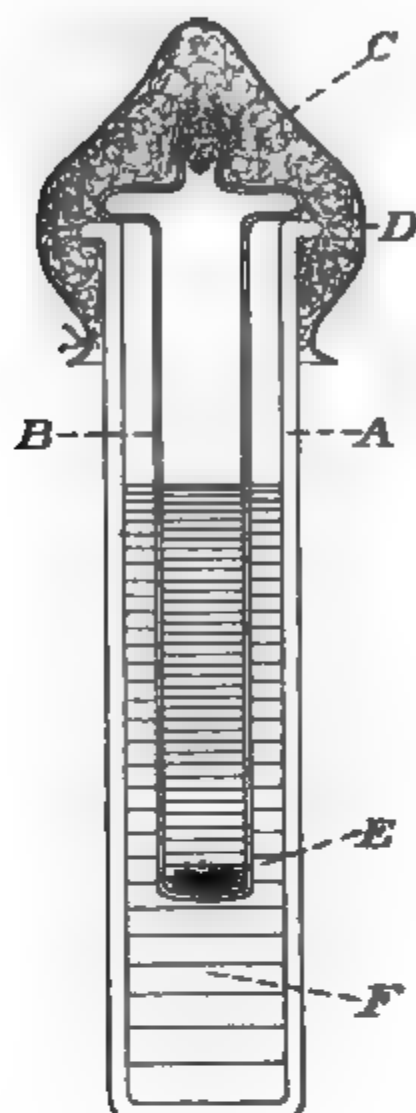
A, glass cylinder; *B*, Chamberland filter; *C*, sterilised cotton wool; *D*, sterilised paper cap tied over the cotton wool; *E*, *Bacillus fluorescens liquefaciens*; *F*, *Bacillus anthracis*.

no spores—while *anthrax was present after this treatment to the extent of many thousands per cubic centimetre of the liquid.*

Clearly, therefore, the anthrax is not poisoned off by the secretions of *B. fluorescens liquefaciens.*

These results suggested the following simple experiment:—

Four sets of test-tubes, properly prepared, were arranged as follows:—

One set (A) received a charge of the green fluorescing liquid obtained by allowing *Bacillus fluorescens* to thoroughly liquefy gelatine, the charge being simply filtered at ordinary temperatures through sterilised filter paper; this merely holds back the large flocculent masses of the Schizomycete, and allows numerous isolated ones to pass.

Set B received an exactly similar charge, but was then put into a beaker of water kept boiling for fifteen minutes.

A third set (C) was charged with equal quantities of ordinary nutrient gelatine and of the above green liquid.

The fourth set (D) was prepared exactly as C, but kept in boiling water for fifteen minutes.

When all were ready, and the heated tubes had cooled to 25—26° C. just sufficient to solidify the gelatine, a large loopful of anthrax spores was placed in each tube, and all four sets put into the dark incubator at 20° C.

The results were less satisfactory, as regards sharpness, than we hoped, but it was clear that the spores of anthrax were still there, and alive in all the tubes, after three weeks.

Finally, the following careful set of experiments were made :—

A quantity of Thames water, collected on the morning of December 10, was at once filtered through a Chamberland porcelain tube at low pressure, the whole apparatus having been very carefully steril-ised. Control experiments showed that this sample of water was almost entirely devoid of water organisms.

Two similar flasks were prepared, and into these 1 litre of the above water was distributed after the water had been infected as follows :—

To the 1 litre of filtered water 1 c.c. of a liquefied gelatine culture of anthrax, consisting almost entirely of rodlets and filaments, but partly of spores, and 1 c.c. of a similar liquid culture of *B. fluorescens liquefaciens* were added, and thoroughly shaken up.

Each charged flask was then placed in the dark, one at 12° C., the other at 20° C., and plates made daily, as shown in Table *y*, which summarises the results.

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Table y.—Behaviour of *Anthrax* and *Bacillus fluorescens liquefaciens* growing together in Filtered Thames Water at 20° C. and 12° C. respectively.

Number of days flask stood.	Temperature at which flask stood. °C.	Date of making plate.	Number of days plate was incubated.	Quantity of water used for plate. c.c.	Diluted or not.	Number of colonies found on plate. Anthrax.	B. fluorescens.	Calculated number of bacteria in 1 c.c original. Anthrax.	B. fluorescens.	Remarks.
0		Dec. 10	2	c.c.	Not	100	100	5,000	5,000	Average = 3968 anthrax and 4918 B. fluorescens per 1 c.c.
0	:	,,	2	1/5	: :	50	90	2,500	4,500	
0	:	,,	2	1/5	: :	100	110	2,500	2,750	
0	:	,,	2	1/5	,,	35	100	1,875	2,500	
1	20	Dec. 11	2	1/10	,,	4000	1,000	100,000	25,000	The plates with 1/10 c.c. and 1/10 c.c. were absolutely beyond counting, but enough was seen to assure me that 100,000 anthrax and 25,000 B. fluorescens is not too high an average.
1	,,	,,	2	1/10	,,	?	?	?	?	
1	,,	,,	2	1/10	,,	?	?	?	?	
1	12	,,	2	1/10	,,	170	64	4,250	1,600	The 3rd plate was too much liquefied, but the numbers were not less than 2nd average 2035 anthrax and 585 B. fluorescens.
1	,,	,,	2	1/10	,,	600	300	5,000	2,500	
1	,,	,,	2	1/10	,,	?	?	?	?	
3	20	Dec. 12	2	1/10	,,	800	3	20,000	75	Again must take average from one plate; the other too far liquefied by *the anthrax colonies*.
3	,,	,,	2	1/10	,,	?	?	?	?	
3	20	Dec. 13	2			...	...	...	...	Plate lost.

Table y—continued.

Number of days flask stood.	Temperature at which flask stood. (°C.)	Date of making plate.	Number of days plate was incubated.	Quantity of water used for plate. (c.c.)	Diluted or not.	Number of colonies found on plate. Anthrax.	B. fluorescens.	Calculated number of bacteria in 1 c.c. original. Anthrax.	B. fluorescens.	Remarks.
3	12	Dec. 13	2	1/10	Not	?	?	?	?	Countless thousands, and badly liquefied, but the type neither that of anthrax nor B. fluorescens.
4	20	Dec. 14	2	1/10	Diluted 5 : 1	?	Total = 1,500	?	Total = 112,500	Most = B. fluorescens, but could not estimate proportions.
4	20	,,	2	1/10	,,	?	Total = 1,200	?	Total = 90,000	Anthrax preponderated, but could not get proportions.
4	12	,,	2	1/10	,,	?	Total = 12,000	?	Total = 900,000	} By far the majority = B. fluorescens in both sets; anthrax present.
4	12	,,	2	1/10	,,	?	Total = 10,000	?	Total = 750,000	
5	20	Dec. 15	2	1/10	,,	?	1,000	?	Total = 75,000	Anthrax present, but much dominated.
5	12	,,	2	1/10	,,	?	?	?	?	Utterly liquefied. Anthrax present, however.
7	20	Dec. 17	2	1/10	No dilution	2700	300	67,500	7,500	? B. fluorescens underestimated.
7	12	,,	2	1/10	,,	?	Total = 5,000	?	Total = 125,000	Majority by far = B. fluorescens, but not typical.
8	20	Dec. 18	2	1/10	,,	400	200	10,000	5,000	Quite typical, and number too low rather than too high.
8	12	,,	2	1/10	,,	?	?	?	?	Too far liquefied to count—many thousands—few anthrax.

10	20	Dec. 20	2	$\frac{1}{5}$:	?	Total = 542	?	Total = 13,550	Almost all *B. fluorescens*, but *some anthrax* recognised.
10	12	"	2	$\frac{1}{2.5}$	?	Total = 3,000	?	Total = 75,000	Foreign forms intruded.
13	20	Dec. 23	3	$\frac{1}{2.5}$	?	Total = 900	?	Total = 22,500	Some *anthrax* present.
13	12	"	3	$\frac{1}{2.5}$	?	Total = 1,200	?	Total = 30,000	
18	20	Dec. 28	3	$\frac{1}{2.5}$	?	Total = 1,100	?	Total = 27,500	Several foreign forms. Anthrax still there.
18	12	"	3	$\frac{1}{2.5}$	?	Total = 5:0	?	Total = 13,750	

The table shows conclusively that the *anthrax was not exterminated, even after eighteen days at either temperature.*

On January 3, the flask kept at 20° C. was heated to 56° C. for twelve hours, and plates made. These plates, after three days' incubation at 20° C., showed that anthrax was still present to the extent of from 110 to 230 per 1 c.c. of the water in the flask.

If the numbers in Tables x, p. 254, and y, p. 295, are put in the form of curves, the ordinates representing the numbers of bacteria, we find in all cases that there is a large and rapid rise to a maximum during the first four days; the climax may be reached during the second day, or the third or fourth, but it is always relatively high, and usually soon reached. Then follows an equally rapid fall during the next twenty-four to forty-eight hours, succeeded by a slower one.

It is interesting to note that the mixed *Bacillus anthracis* and *B. fluorescens liquefaciens* behave very similarly *as a whole* at the lower temperature, 12° C., which is not favourable to the anthrax; but at the *higher* temperature of 20° C. it looks as if disturbances of various kinds occur which lead to a very different and irregular curve.

It seems almost certain that temperature is not the only factor at work here, for, although its effects are very distinct, as shown by comparing Tables x, y, and z, there must be other circumstances concurring to account for the very different heights of the curves, and times taken to reach this maximum.

The extreme rapidity of the main ascent, when once it begins, suggests that a moment arrives when the ferment actions which must be supposed to render the food materials available are at their best; if this is so, the sudden fall may well be due to two causes—(1) the exhaustion of the available oxygen, and (2) that of the food materials themselves.

The actual height of the maximum— *i.e.*, the numbers of bacteria then sustained by the medium—may well be supposed to depend on a number of factors, *e.g.*, the quantity of food materials, especially organic, present in the water, the number of bacteria in the water at the outset, and, of course, the nature of both, being amongst the most important of these.

Obviously the whole subject is extremely complex, but we are inclined to think that valuable information could be got by extensive comparative examinations at constant temperatures of waters of known chemical composition infected with definite quantities of two known organisms, the behaviour of which should be studied at intervals of twelve hours if possible.

Experiments with Oxygenated (Aërated) Thames Water.

In order to test the action of combined movement and aëration, we employed the following means:—A flask of 1½ litre capacity, and with a side arm tubulure, was half filled with the water to be experimented on, and its neck fitted with a caoutchouc stopper through which passed a long, narrow glass tube; the lower end of the tube was drawn to a point and passed to the bottom of the flask; the upper end was curved back on itself and carefully packed with successive plugs of sterilised cotton wool, and the end covered with a large tuft of the same. Preliminary trials convinced us that if this apparatus is properly sterilised, and carefully used, air may be drawn through the cotton-wool filter for two or three weeks without contamination of the contents. The accompanying figure represents the apparatus.
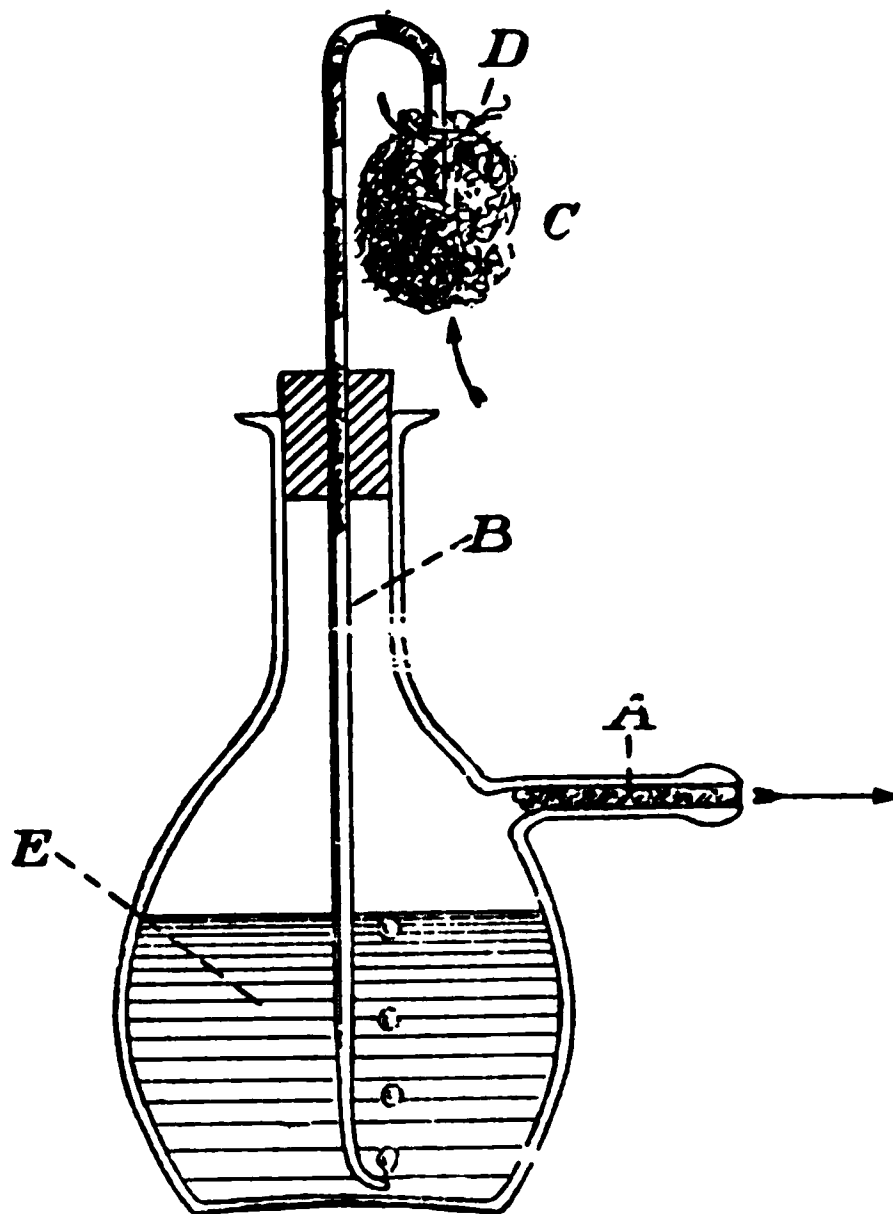
A, lateral tubulure plugged with cotton wool and attached to pump; *B*, curved tube plugged at intervals with cotton wool to filter air which passes into liquid below; *C*, large wad of cotton wool, tied on to air tube at *D*; *E*, water containing bacteria to be experimented with.

In the following series of experiments, one flask was aërated in the manner described for six days, and an exactly similar flask kept standing quietly by its side under exactly similar conditions otherwise. Both flasks contained Thames water, from the same collection,

and placed in action within a few hours of its removal from the river. The temperature of the room was 15—16° C., and remarkably constant day and night. The flasks were exposed to diffused daylight, and occasionally to the light of a Swan lamp.

After the six days the two flasks showed the following results on bacteriological analysis by means of gelatine plate cultures, using 1, 3, and 6 drops per plate, and diluting with 10 vols. sterile water to 1 vol. from flask. The numbers given are the calculated averages, and it is possible that more extended observations would, perhaps, alter them slightly.

State of water.	Number of drops in culture.	Total bacteria per 1 c.c. original.	Proportion of liquefyiug bacteria.	Remarks.
Aërated	1	35,640	26 : 64	
Non-aërated ..	1	17,695	24 : 23	
Aërated	3	33,000	26 : 64	
Non-aërated ..	3	15,840	1 : 3·5	
Aërated	6	uncountable	..	} Liquefied too rapidly for estimation.
Non-aërated ..	6	„	..	

It must be remembered that in such experiments as these, the rapid development of the liquefying organisms is always the chief trouble, since we cannot allow the plates to incubate long enough to bring forward all the liquefying forms, on the one hand, while the liquefaction overpowers the young non-liquefying colonies on the other: consequently we lay no stress on the last column.

As regards the direct effect of the aëration, we believe the third column does express it more or less accurately, though of course the reply is always possible that we have no absolute guarantee that no aërial forms were filtered into the flask.

We propose to extend this inquiry at a later period, and merely put forward our results so far as tentative, and by no means devoid of interest and suggestiveness.

We next resolved to extend the comparison between aërated and non-aërated cultures to flasks of Thames water treated *exactly* as in the foregoing series except that we first infected both flasks with anthrax.

We employed a gelatine culture of the anthrax, selecting a tube of rapid growth, and in which the gelatine was completely liquefied, and used a relatively very large quantity (5 c.c. to the litre of water) *and of course* a correspondingly large quantity of gelatine food-material. *This was* done purposely in this first experiment, to en-

sure a distinct advantage to the anthrax in its struggle with the competing water forms.

The results are again expressed in calculated averages, in the following table. As before, the aëration was conducted for six days at 15—16° C., a temperature at which the spores can germinate.

State of water.	Number of drops in culture.	Total bacteria per 1 c.c. original.	Estimated proportion of anthrax to other bacteria.	Remarks.
Aërated......	1	Many thousands	1 : 10	
Non-aërated ..	1	Countless „	1 : 10	As far as could judge the proportions were similar.
Aërated......	3	..	1 : 20	
Non-aërated ..	3	..	..	Impossible to estimate.

Here it must be admitted that we failed in our attempt to ascertain any effects of the aëration on the anthrax as compared with the water organisms.

We, meanwhile, altered the course of the inquiry as follows :—Each flask, aërated and non-aërated, was placed, at the end of the six days, as soon as the samples had been removed for plate cultures, at 60° C. for twenty-four hours, and plates then made to ascertain, if possible, what had happened to the anthrax—*i.e.*, to see if spores had been formed, and to what extent in the two cases.

The following table summarises the results :—

State of water.	Number of drops used.	Calculated number of anthrax per 1 c.c. original.	Remarks.
Aërated..............	1	120,000	
Non-aërated	1	99,000	
Aërated.............	6	100,000	
Non-aërated	6	125,000	

So far as the quantitative results go, we regard the experiments here summarised as failures, because no stress whatever may be laid on the actual numbers until we have made a larger series on these comparative lines.

We do think, *however*, that the results are valuable in another

sense; for they show quite clearly that (1) the oxygenation and movement of the water for six days in diffused light does not sensibly reduce the anthrax if it passes into the spore stage, and (2) does not by any means eliminate the element of a struggle with normal water forms.

We regard the subject as well worth more exact study, and especially along lines to determine more accurately the relative direct effects on the various organisms in the water.

The following series was designed as a continuation of this inquiry.

One litre of fresh Thames water was infected with 5 c.c. of a gelatine culture of virulent anthrax, the culture being one day old, and distributed into three flasks A, B, and C, in equal quantities. The flask A was aërated, as before, B stood quietly by its side: these two flasks were exposed to ordinary diffused daylight, while C stood quiet in a south window, exposed to what sunlight could fall on it in November.

At the outset, we examined the infected water, and found the proportions of organisms as follows :—Water organisms = from 370 to 450 per 1 c.c., and anthrax from 150,000 upwards per 1 c.c.

After seven days, our examination gave the following results, tabulated as averages :—

State of flask.	Number of drops in culture.	Total bacteria per 1 c.c. original.	Proportion of anthrax to other organisms.	Remarks.
Aërated 7 days....	1	4,000,000	1 : 35	The numbers were so enormous that we could make nothing of the 3-drop cultures.
Non-aërated 7 days	1	6,000,000	1 : 50	
Insolated 7 days...	1	6,000,000	?	

After taking the samples for direct culture, we placed similar samples of each flask at 60° C. for twenty-four hours, and made plates again, with the following results :—

State of water.	Number of drops used.	Number of anthrax per 1 c.c. original.	Remarks.
Aërated	1	270,000	
Non-aërated	1	153,000	
Insolated	1	117,000	
Aërated	3	140,000	
Non-aërated	3	130,000	
Insolated............	3	135,000	

Here, again, we abstain from dwelling too much on the quantitative results, though, so far as they go, they suggest that aëration favours the sporification or preservation of the anthrax, while insolation—even feeble—tends to destroy the spores. But the positive qualitative result is obvious, that the anthrax if it passes over into the spore stage in these waters becomes, thereby, to a great extent removed from the direct competition with the water organisms.

Experiments on the Action of Light on Bacillus anthracis.

It is abundantly evinced by experiments that direct insolation in some way leads to the destruction of spores of *Bacillus anthracis*, and in so far the results merely confirm what had already been discovered by Downes and Blunt in 1877 and 1878.[*]

From the fact that an apparent retardation of the development of the colonies on plates exposed to light was observed several times under circumstances which suggested a direct inhibitory action of even ordinary day-light, the author went further into this particular question with results as startling as they are important, for if the explanation given of the phenomena observed in the following experiments turns out to be the correct one, we stand face to face with the fact that by far the most potent factor in the purification of the air and rivers of bacteria is the sun-light. The fact that direct sun-light is efficacious as a bactericide has been long suspected, but put forward very vaguely in most cases.

Starting from the observation that a test-tube, or small flask, containing a few c.c. of Thames water with many hundreds of thousands of anthrax spores in it may be entirely rid of living spores by continued exposure daily for a few days to the light of the sun, first shown for water by Straus ('Soc. de Biologie,' 1886, p. 473), and that even a few weeks of bright summer day-light—not direct insolation —reduces the number of spores capable of development on gelatine,

* See p. 237 of "First Report to the Water Research Committee of the Royal Society" ('Roy. Soc. Proc.,' vol. 51, 1892) for the literature on this subject up to 1891.

it seemed worth while to try the effect of direct insolation on plate-cultures, to see if the results could be got more quickly and definitely.*

Preliminary trials with gelatine plate-cultures at the end of the summer soon showed that precautions of several kinds were necessary. The direct exposure of an ordinary plate-culture to the full light of even a September or October sun, especially in the afternoon, usually leads at once to the running and liquefaction of the gelatine, and although the exposed plates eventually showed fewer anthrax colonies than similar plates not exposed, the matter was too complicated to give satisfactory results. Obviously one objection was that the spores might have begun to germinate, and the young colonies killed by the high temperatures.

Experiments made in October with gelatine plates wrapped in black paper, in which a figure—a square, cross, or letter—was cut, also led to results too indefinite for satisfaction, although it was clear in some cases that if the plates lay quite flat, the illuminated area was on the whole clear of colonies, while that part of the plate covered by the paper was full of colonies.

But another source of vexation arose. After the plates had been exposed to the sunlight for, say, six hours, it was necessary to put them in the incubator (20—22° C. was the temperature used) for two days or so, to develop the colonies, and in many cases it was observed that by the time the colonies were sufficiently far advanced to show up clearly, liquefaction had extended so far as to render the figure blurred and doubtful.

Stencil plates of zinc were employed with, at first, equally un-certain results. The stencil plate was fixed to the bottom of the plate culture, outside, and every other part covered with blackened paper: the plate was then placed on a level surface, the stencil-covered face upward, and exposed to the direct sunlight. As before, the gelatine softened and in many cases ran, and the results were uncertain, though not altogether discouraging.

In November it was found that more definite results could be obtained, and the problem was at last solved.

Meanwhile it had already been found possible to obtain sun prints in the following way with agar plates. Ordinary agar was heated and allowed to cool to between 50° and 60° C., and was then richly in-fected with anthrax spores, and made into plates as usual. Such plates were then covered with a stencil plate on the lower face—the stencil plate being therefore separated from the infected agar only by the glass of the plate—and wrapped elsewhere closely in dull

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* It appears that Buchner ('Centr. f. Bakt.,' vol. 12, 1892) has already done this for typhoid, and finds the direct rays of the summer sun quite effective.

black paper, so that, on exposure to the sun, only the cut-out figure or letter allowed the solar rays to reach the agar.

Such plates were then exposed to the direct rays of the October sun for from two to six hours; or they were placed on the ring of a retort-stand, stencil downwards, and the sun-light reflected upwards from a plane mirror below.

After the insolation, these plates were incubated for at least forty-eight hours at 20° C., and on removing the wrappers the colonies of anthrax were found densely covering all parts of the plate except the area—a letter or cross, &c.—exposed to the sun-light. There, however, the spores were killed, and the agar remained perfectly clear, showing the form of a sharp transparent letter, cross, &c., in a ground-work rendered cloudy and opaque by the innumerable colonies of anthrax.

Experiments proved that this was not due to high temperature, for a thermometer with its bulb next the insolated glass rarely rose beyond 14° to 16° C., and never beyond 18° C., and even if the thermometer did not record the temperature inside the plate, this can scarcely have been much higher.

As long as this latter point remained uncertain, however, the

Fig. 1.
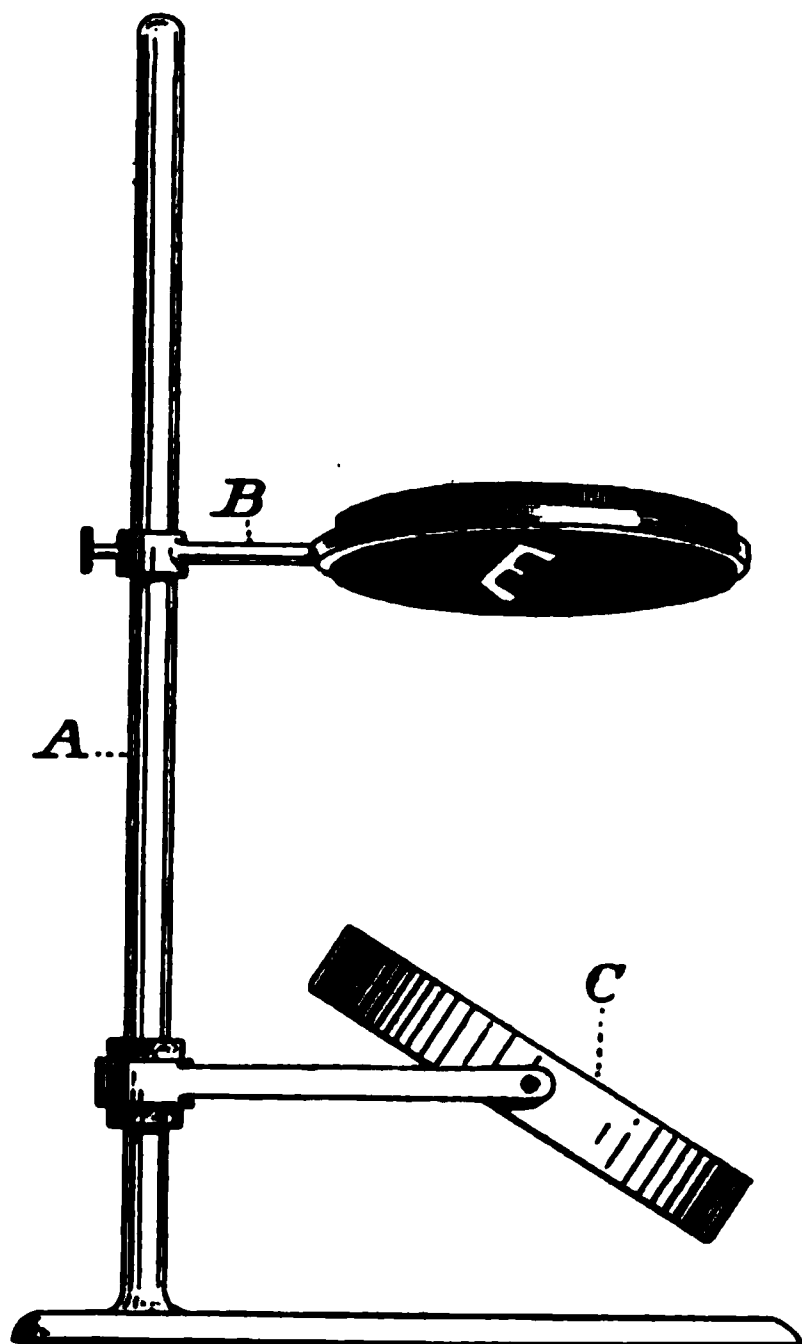
experiments could not be regarded as satisfactory; whence it was necessary to again have recourse to gelatine cultures. The gelatine employed began to run at 29° C., and in November it was found that such plates exposed outside, either to directly incident sunshine, or to directly reflected rays, showed a temperature of 12° to 13° C. at the insolated glass surface, and even five to six hours' exposure caused no running of the gelatine.

The following experiment may be selected as a type of the rest :— A (fig. 1) is the upright of an ordinary retort-stand; on the ring B rested a gelatine plate-culture of anthrax spores, covered with black paper everywhere except the cut-out letter E, seen on its lower face. C was an ordinary plane microscope-mirror, with its arm fitted to a cork on A.

The whole was placed in the middle of a field at Cooper's Hill at 9.30 A.M. on Wednesday, November 30, and exposed to the clear, but low, sunshine which prevailed that day, the mirror being so arranged (from time to time as necessary) as to reflect the light on the E the whole period, until 3.30 P.M., when the plate was removed and placed in the dark incubator at 20° C. On the following Friday —i.e., after less than forty-eight hours' incubation—the letter E stood out sharp and clearly transparent from the faint grey of the rest of

Fig. 2.
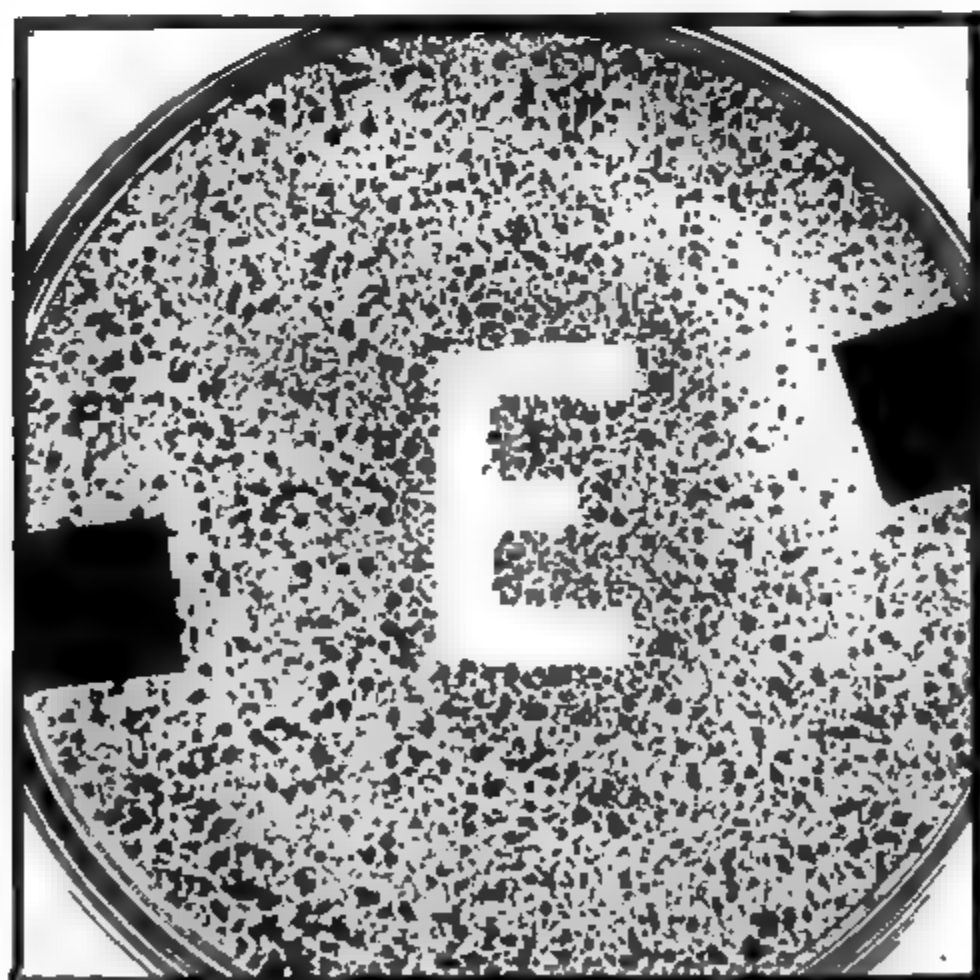
the plate of gelatine. Not a trace of anthrax could be found in the clear area, even with the microscope, while the grey and almost opaque appearance of the rest of the plate was due to innumerable colonies of that organism which had developed in the interval.

It was impossible to incubate the plate longer for fear of liquefaction, whence the sceptical may reply that the anthrax exposed to light was only retarded; the experiments with agar show that such is not the case, however, and that if the insolation is complete the spores are rendered incapable of germinating at all, as proved by removing pieces of the clear agar or gelatine and attempting to make tube cultures from them: in all cases where insolation is complete they remain sterile.

The chief value of these gelatine plate exposures in November, however, is that they prove conclusively (1) that the rays of a *winter* sun are capable, even if reflected, of killing the spores, and (2) that it is really the solar rays which do this directly, and not any effect of a higher temperature, since the gelatine remains solid throughout.

Experience has shown, however, that some precautions are necessary in selecting the anthrax cultures employed for these experiments with gelatine. The light certainly retards or kills (according to its intensity or the length of exposure) virulent spores, but if one takes the spores, mixed with vegetative bacilli, direct from a thoroughly liquefied gelatine culture, or from a bouillon culture, the plates are apt to be liquefied too rapidly for the proper development of the light print, evidently because so much of the liquefying enzyme is carried in when inoculating the plates. The same danger is run when active bacilli alone are employed.

The best method of avoiding these disadvantages has been found to be the following, and it has the additional merit of enabling us to prove, beyond all doubt, that the ripe spores of *Bacillus anthracis* are really inhibited or killed by sunlight.

A few c.c. of sterile distilled water in a tube are thoroughly saturated with the anthrax spores taken from an old culture which has never been exposed to light, and the tube placed for twenty-four hours at 56° C.; this kills all immature spores, bacilli, and enzymes, and leaves us with a crop of the most resistant and fully matured virulent spores.

Experiments with such spores have been made to determine the relative power of the different rays of the spectrum to destroy the anthrax.

It is necessary to note first, however, that in experimenting with the electric light, although but few exposures have been made as yet, it is evident that its effects are feebler than those of the winter sun.

At present it has only been possible to observe that the inhibiting effects are stronger at the blue end of the spectrum than at the red,

and exposures to sunlight passing through coloured glasses confirm this result; but the observations are being continued in the hope of getting a perfectly sharp record of the effects of each set of rays.

The following series of experiments are quoted in detail, because they teach several details of importance, in addition to proving the main fact.

On December 7 three gelatine plates and five agar plates were prepared with spores from a very vigorous and virulent agar tube of anthrax. The spores, which were quite mature, were not subjected to heat, but simply shaken in sterile water to wash and separate them thoroughly.

The three gelatine plates were made at 35° C., the agar plates at 60° C., neither of which temperatures could injure the ripe spores.

The three gelatine plates were labelled $p\,1$, $p\,2$, and $p\,3$, and the agar plates $p\,4$ to $p\,8$ in order.

Immediately after making the plates, all were exposed to the December sun, except plates $p\,4$, $p\,5$, and $p\,6$, and this was done as follows:—In each case the plate had a stencil plate with a cut-out letter on its lower face, and arranged as described above (p. 304).

$p\,1$, a gelatine plate with a *large* letter M, was exposed, face down, to the light reflected from a mirror (see fig. 1) for three hours on December 7, and for four hours on December 8, the interval being passed in a cold room (t about 8—9° C.), and then incubated at 20° in the dark.

$p\,8$ was treated in exactly the same manner. But this was an agar plate with a *large* W.

$p\,2$, a gelatine plate with a *large* H, was exposed and treated in the same way, except that no mirror was used, the latter being upwards towards the sun.

$p\,3$, a gelatine plate with a *large* B, was similarly exposed, face up, but a plane mirror arranged to reflect light down upon it.

$p\,7$, an agar plate with a *large* E, was treated exactly as the last.

There now remain the three agar plates, $p\,4$, $p\,5$, and $p\,6$, to account for.

$p\,4$ was placed forthwith in the dark incubator at 20° C.

$p\,5$ and $p\,6$ were kept for eighteen hours in a drawer, the average temperature of which is almost 16° C., and were not exposed till next day (December 8), when they lay for five hours, face upwards, and with a mirror above them. $p\,5$ had a *small* E, and $p\,6$ a broad but small I, to let the light in.

After exposure, these also were put in the same incubator with the others.

Nothing was visible to the unaided eye on these plates (except $p\,4$) until the 11th instant, though the microscope showed that germination was proceeding on the 10th. The plate $p\,4$, however, had a

distinct veil of colonies all over it on the 9th, and this had developed to a dense typical growth by the 11th.

On December 11, at 10 A.M., the state of affairs, as regards the exposed plates, was as follows :—

p 5 and *p* 6 showed each a sharp transparent letter—E and I respectively—of clear agar in a dull grey matrix of strong anthra2 colonies, which covered all the unexposed parts of the plate.

p 1, *p* 2, and *p* 3 showed in each case a perfectly clear central patch, about 1½ inches diameter, with anthrax colonies in the gelatine around. These anthrax colonies were the *larger and more vigorous the more distant they were from the clear centre.* In other words, the anthrax spores had begun to germinate, and the colonies were growing more vigorously, in centripetal order.

On *p* 7 and *p* 8 germination was beginning, but the colonies were as yet too young to enable one to judge of the results.

The first point of interest is to account for the pronounced results in the plates *p* 5 and *p* 6, and the want of sharp outlines in *p* 1, *p* 2, and *p* 3, and the explanation seems to be that, owing to the plates 5 and 6 having laid over night at 16° C., the spores began slowly to germinate out, *and were consequently in their most tender condition* when exposed to the sunlight next day.

The peculiar centripetal order of development of the colonies on plates *p* 1, *p* 2, and *p* 3 gave rise to the following attempt at explanation. After observing that the clear space in the middle was not due to the centre of the plate being raised, and the infected gelatine having run down to the periphery—a possible event with some batches of Petrie's dishes—it was surmised that the *large* letters employed might give the clue.

This was found to be the case. The solar rays on entering the plate were largely reflected from the glass lid of the plates, and so produced feebler insolation effects on parts of the plate around the letter : these effects were naturally feebler and feebler towards the margin, and so the inhibitory action became less pronounced at distances further and further removed from the centre. Those spores, therefore, which were nearest the periphery germinated out first, and those nearer the centre were retarded more and more in proportion to their proximity to the insolated letter.

That this is the correct interpretation of the facts follows clearly from the further behaviour of the above plates.

At 10 P.M. on the 11th—*i.e.*, twelve hours after the morning examination—the plates *p* 1, *p* 2, and *p* 3 exhibited their respective letters M, H, and B quite clearly, in the grey matrix of anthrax which had rapidly developed in the interval, and excepting a slight want of sharpness in the H of *p* 2, the results could hardly have been more satisfactory.

In *p* 7 and *p* 8 the *very faint* outlines of the letters were also showing.

On the 12th, at 8.30 A.M., the gelatine plates had begun to run, and although the M of *p* 1 was still intact, and very well marked, *p* 2 had liquefied completely, so that the H was a clear patch with blurred outlines in the centre. *p* 3 still showed the outlines of the B, but it was impossible to keep it longer.

The main point was definitely established, however, and the treatment of the plates proves conclusively that the spores are not killed by high or low temperatures, *but by the direct solar rays.*

These experiments are being continued in order to answer some other questions in this connexion.

The gelatine and agar after such exposures as have been described are still capable of supporting a growth of *B. anthracis* if fresh spores are sown on them, whence the effects described are not merely due to the sub-strata being spoilt as food material.

That the action of the light is *direct* on the spore, and not due to any reaction from the medium, I have recently shown by the following new method :—

A thin layer of *dried spores only*, spread on glass without food materials, shows the letter as in the experiments on pp. 305—306, *if a slab of solidified agar is placed on the film of spores after exposure*, and the whole incubated; whereas the reciprocal treatment—where the *agar alone* is exposed, and then laid on a *film of spores*—yields negative results, the spores germinate equally well all over.[*]

Conclusions to Part II.

The following conclusions are to be drawn from the results of the experiments recorded in this Part II of the Report :—

1. Thames water, like all open waters, contains a variable number of bacteria at all times (pp. 244—256).

2. The actual numbers of these bacteria are not great, but comparisons show that there are more in the Thames in December than in March, and fewer still in June (pp. 246, 252, 254).

3. There are no reasons for supposing any of these water bacteria to be pathogenic, and some of them have been recognised as known saprophytes (pp. 247, 280, 285—290).

4. In agreement with the universal experience of those observers

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[*] This proof that the action of the light is *direct* on the spores is opposed to Roux's conclusions ('Ann. Inst. Pasteur,' 1887, pp. 445—452), and in support of those of Arloing ('Compt. Rend.,' vol. 104, 1887), and of Janowski ('Centralbl. für Bakteriol.,' 1890, Nos. 6—8). These authors worked with less perfect methods, however, and Janowski's results concern typhoid only. I have given more extensive results in the paper read to the Royal Society on February 16, 1893.

who have attended to the question, we find these water bacteria to multiply with astounding rapidity if the water is allowed to stand for a few days; the maximum numbers are reached in from one to four days as a rule, and the curve itself is exceedingly steep and sharp (pp. 249—256).

5. Our observations go to show that the rapidity of increase and the maximum numbers reached depend on various factors—temperature, oxygen supply, the amount and quality of the food materials in the water, and the nature and numbers of the bacteria concerned (pp. 254, 291, 295, 299—302).

6. We have experimented with the waters, chiefly of the Thames, in three conditions, viz. :—(1) fresh from the river and not subjected to any treatment; (2) deprived of all of the above bacteria by filtration through porous porcelain; and (3) sterilised by heat. (4) Experiments have also been made with distilled water. We regard the four conditions of water referred to as essentially different one from another (pp. 259—263, 266—273, 275—278).

7. We have employed such waters to test the power of resistance of *Bacillus anthracis* (anthrax) both in the form of spores and of vegetative bacilli, and also in the asporogenous state (pp. 256—285).

8. We have, moreover, employed the anthrax in a virulent and in a weakened condition (pp. 266—273).

9. And we have experimented under various conditions as regards (1) time, (2) temperature, (3) light, (4) the presence of other organisms, with the following results briefly summarised :—

10. Neither as spores nor as bacilli—weak or strong—is anthrax killed forthwith in any of the waters under any of the conditions tested; but the spores are immensely more resistant than the bacilli (pp. 259—285).

11. In the dark, and at moderate temperatures, the spores of anthrax retain their powers of germination and infection for many months—we have proved up to eight months—in any of the waters referred to (pp. 278—283).

12. In direct sunlight, however, the spores in the waters undergo rapid destruction, depending on the intensity of the insolation and the time of exposure (pp. 279, 283, 303).

13. That this destruction is directly due to the light-rays, especially at the blue end of the spectrum, and not to a rise of temperature fatal to the spores, is definitely proved by the experiments with gelatine and agar plates. These experiments also demonstrate conclusively that the bactericidal action is really *direct*, and not due indirectly to the action of the solar rays on the medium (p. 310).

14. The value of the experiments on the bactericidal action of direct sunlight on the spores of *B. anthracis* is the more important when it is reflected that the most decisive results have been obtained

by exposure to the rays of a *winter sun* (November and December), at temperatures so low that no question of heat can come into consideration; moreover, the experiments prove that the bacteria spores are really killed, and not merely retarded (p. 307).

15. That we have here an essential part of the explanation of numerous phenomena cannot be doubted, *e.g.*, the purity of shallow running waters, and the steady diminution of bacteria from our rivers, lakes, &c.; while, conversely, the suspension of solid particles, rendering water turbid, may obviously react on their bacterial life by intercepting the sun's rays.

16. These experiments also suggest how necessary it is to conduct all cultures of such bacteria in the dark; and bring vividly before us the importance of direct sunlight in our streets and dwellings, &c., and in numerous circumstances of life.

17. In no case have we succeeded in showing that *Bacillus anthracis* multiplies to any considerable extent in the form of vegetative bacilli in the above waters, unless appreciable quantities of organic food materials are added and the temperature is raised to above 12° C. So far as we can decide, the bacilli either die off in the course of the first day or two, or, if the conditions are favourable, they form spores (pp. 272—273). I regard this question as to the power of the bacilli to multiply or form spores in the water as the most important from a hygienic point of view; unfortunately it is also by far the most difficult one to answer.

18. Aëration and consequent disturbance of the water containing anthrax does not destroy nor appreciably affect the latter (pp. 299—302). Nor does the presence of ordinary green Algæ in the standing water seem to affect it, or, at any rate, not more than can be explained by the diffused light necessary for the Algæ (pp. 278—283).

19. Experiments prove that anthrax by no means succumbs easily, if at all, in the struggle for existence with *Bacillus fluorescens liquefaciens*, one of the commonest of the Thames bacteria, and remarkable for its aërobism and liquefying powers (pp. 290—298).

20. Whether the result will be the same with other bacteria selected for antagonism remains to be shown; several other Thames bacteria have been isolated, and are being studied in detail to the end that their effects may be tested.

PART III.

Joint Conclusions arrived at by both Authors.

After carefully comparing the results, we beg to submit to the Committee the following conclusions at which we have arrived on the subject of inquiry :—

1. The waters both of the Thames and of Loch Katrine normally contain a number of different forms of micro-organisms, some of which have been isolated and described (pp. 178—180, 186—191, 244—246, 285).

2. These bacteria are, as far as our comparisons have been pursued, more numerous in the water of the Thames than in that of Loch Katrine, and the numbers in the Thames water at least have been shown by us to be subject to well-marked seasonable variations, being usually much greater in winter than in summer (pp. 178—180, 246). This relationship is probably due to the Thames water in dry weather being to a large extent derived from springs, whilst after rain, especially in winter, it receives considerable accessions of surface water rich·in bacterial life and the organic materials which promote the growth and multiplication of micro-organisms (pp. 178—180, 225).

3. Hitherto no pathogenic bacteria have been found in the Thames water, either by other observers or ourselves (p. 258).

4. In agreement with the universal experience of all observers who have given attention to the subject, we have found that the water bacteria, both of the Thames and of Loch Katrine, multiply with astounding rapidity when these waters are allowed to stand for a few days, a maximum being rapidly reached, which is followed by a corresponding, although less precipitate, decline (pp. 190, 191, 226, 249, 254.)

5. An adequate explanation of this remarkable multiplication has not yet been given, and is the more difficult to find, inasmuch as it has been shown that the same phenomenon occurs in the case of waters, like those of deep wells, which are almost wholly destitute of organic matter. Again, although oxygenation and a high temperature undoubtedly accelerate this multiplication, it even takes place to a surprising extent at the low temperature of a refrigerator.

6. We have experimented with the waters both of the Thames and Loch Katrine in the three following conditions :—(1) in their natural state as derived from the river and loch (p. 246); (2) sterilised, or

deprived of all their bacteria, by filtration through porous porcelain; and (3) sterilised by heat, 100° C.; (4) experiments have also been made with distilled water. We regard these several conditions of the waters referred to as essentially different one from another (pp. 184, 185, 213, 214, 228).

7. Into such waters we have introduced the *Bacillus anthracis* (anthrax) in the form of (*a*) vegetative bacilli (pp. 256—278); (*b*) spores (p. 278); (*c*) in the "asporogenous" variety (pp. 274—278); contrasting also the effect of using large and small quantities of this micro-organism, and of employing it in a virulent and an attenuated or weakened condition respectively (pp. 184, 213, 228).

8. The principal factors to which we have devoted attention in these experiments have been (1) the temperature at which the infected waters were maintained; (2) whether they were exposed to light or preserved in darkness; (3) the presence or absence of other organisms besides anthrax in the waters (pp. 184, 214, 228; all experiments were conducted in the dark, excepting when otherwise stated, 40, 45, 54, 57, 59, 67, 69, 71; 92, 119, 126—134).

9. We will in the first instance call attention to the results which we have obtained in our experiments with these spores.

We found that the behaviour of the spores was very different, according as they were introduced into the unsterilised or sterilised waters respectively (pp. 181—243).

10. In the sterilised waters their behaviour was practically uniform, irrespectively of whether Thames or Loch Katrine water was employed, irrespectively of whether the water was sterilised by filtration through porcelain or by steam, and also irrespectively of whether the waters were preserved at a summer temperature of 18—20° C., or in the refrigerator at 4—9° C. In all cases the spores retained both their vitality and their virulence for many months. After this prolonged residence in these sterile waters, they were recognisable by cultivation in either the same or in only slightly diminished numbers from those in which they were originally introduced into these waters. These infected sterile waters, after standing for upwards of seven months, were also invariably fatal to the animals into which they were inoculated (pp. 200, 219, 232, 234, 238).

11. The same results with these infected sterile waters were obtained irrespectively of whether they were preserved in absolute darkness or freely exposed to diffused daylight. Direct sunshine, on the other hand, was rapidly fatal to the anthrax spores in these waters within 84 hours. In the waters so insolated anthrax could not be detected by cultivation, and animals inoculated with these waters remained alive. But in order to make absolutely certain that the *anthrax* spores were quite extinct in these insolated waters, we incu-*bated them with some* sterile broth, so that if only a single spore had

remained in the water it would have multiplied abundantly; but the waters, even after this treatment, proved innocuous to animals (pp. 209—212).

12. The striking results obtained by direct insolation at low temperatures in the open air in winter bring vividly before us the extreme importance of this bactericidal action of direct sunlight, for they show conclusively that the action is direct, and not due to any rise of temperature from the heat rays (pp. 303—310). Other facts and their consequences are given in the conclusions to Part II.

13. We found that when the spores were introduced even in very large numbers into unsterilised waters in their natural condition, they were often no longer recognisable by the ordinary cultivation methods after the lapse of a few days, and it was only by resorting to special methods of detection that the anthrax spores could be discovered. By employing these special methods, however, we have conclusively shown that the number of anthrax spores undergoes a continuous decline in such unsterilised waters, and thus presents a marked contrast to the persistence of the numbers in the sterile waters referred to above. Notwithstanding this decline in the number of anthrax spores, their presence could still be demonstrated many months after their introduction into the Thames water, and this infected water still retained its power of killing animals after upwards of seven months (pp. 278—283), either by direct inoculation (when large numbers of anthrax spores had been originally introduced) or after preliminary incubation with sterile broth (which had to be resorted to when only a small number of anthrax spores was originally introduced, pp. 192—200, 214—219, 229—231, 236—238).

In the case of the Thames water we found but little difference in the result when the waters were kept at winter and summer temperatures respectively, but in the case of the Loch Katrine water a marked difference was exhibited in this respect, for at the summer temperature (18—20° C.) the anthrax spores underwent such rapid degeneration that after three months they were no longer recognisable by cultivation. Moreover, the water kept at the summer temperature proved in every case to be no longer fatal to animals when inoculated directly, and out of two such specimens of water which had been specially incubated with broth for the purpose of revivifying any lurking anthrax spores that might remain, only one became virulent, showing that in the other at least complete extinction of the anthrax spores had taken place. It is suggested that this comparatively rapid destruction of the anthrax spores in unsterilised Loch Katrine water at 18—20° C. is due to the elaboration of bactericidal products by the water bacteria, and not to the character of the moorland water itself, for in the sterile Loch Katrine water the destruction of anthrax *spores* at this temperature did not take place (pp. 228—239).

14. The results obtained with the anthrax spores in the un-
sterilised waters were not influenced by whether these waters were
preserved in darkness or exposed to diffused daylight. Exposed to
direct sunshine, however, the anthrax spores were rapidly destroyed,
not *more* rapidly, however, than in the sterilised waters under the
same exposure (pp. 204—213).

15. In experiments made in order to test the nature of the conflict
between anthrax and particular forms of water bacteria, the *Bacillus
fluorescens liquefaciens* (Flügge) was employed in pure cultivation
along with anthrax in approximately equal proportions. The results,
however, show that this saprophyte, at any rate, has not the power of
rapidly destroying the anthrax spores; indeed, there was no evidence
that either it, or its products, act prejudicially on anthrax spores at
all (pp. 290—298).

16. In connexion with the antagonistic interests of the anthrax on
the one hand and the several kinds of water organisms on the other,
it is worthy of note that in one experiment in which anthrax spores
were introduced into unsterilised Thames water exposed freely to
daylight, and in which in addition to water bacteria there was also
present a quantity of small algæ, the anthrax spores survived the
conflict with these competing forms for upwards of seven months,
although enormously reduced in numbers and much impaired in
virulence (pp. 278, 283).

17. To summarise our results with anthrax spores in one sentence,
we may state generally that there is one natural agency at least
which is capable of destroying them in surface waters to which they
may have gained access, viz., the action of direct sunshine on the
organism. Whether the activity of. water bacteria may be added as
a second bactericidal agent is not definitely determined, but, in any
case, of these two influences the sunshine is by far the more rapid
and the more potent, though its sphere may be much more re-
stricted.

18. *Behaviour of Anthrax Bacilli.*—As regards the behaviour of
anthrax bacilli free from spores, it should be pointed out that we
have only experimented with such spore-free bacilli obtained from
artificial cultures, and not with those derived directly from the
organs of an animal dead of anthrax. We have in many cases found
that the bacilli obtained from artificial cultures behave in essentially
the same way when introduced into water as do the spores under the
same circumstances, and apparently for the reason that the bacilli
introduced rapidly produce spores in the water, and the subsequent
phenomena thus become identical with those which we have already
discussed above (pp. 260, 272, 278).

19. Some of the evidence points to the possibility of the multiplica-
tion of the bacilli in waters containing more than the usual amount

of organic materials; but in no case does it support the view that *Bacillus anthracis* can live and multiply like a water bacterium in ordinary waters (p. 273).

20. As regards the behaviour of that variety of anthrax bacillus which is known as "*asporogène*," and which is incapable of forming spores under any known circumstances whatever, our experiments are not yet sufficiently advanced to warrant any conclusions being drawn from them at present (pp. 274—278). The great manipulative difficulties of experiments with spore-free bacilli have already been pointed out, and no one has, so far as we know, as yet, overcome them.

In conclusion, we would point out that the chief hygienic interest of our investigation is centred in the behaviour of the anthrax spores, which, as we have already pointed out, may be regarded as representative of the extreme limit of endurance possessed by pathogenic bacteria; on the other hand, the most important question to be examined was whether the bacilli of anthrax can grow and multiply or form spores in such waters, and our results point to this being possible only under special conditions. We trust, therefore, that the information which we have collected, both from our own experiments and from the published results of other observers, concerning the behaviour of these hardy anthrax spores, may serve as a basis for practically assessing the higher limit of possible vitality which may be exhibited by pathogenic micro-organisms gaining access to potable water.

Account of the appropriation of the sum of £4,000 (the Government Grant) annually voted by Parliament to the Royal Society, to be employed in aiding the Advancement of Science (continued from vol. 1, p. 246).

November 30, 1891, to March 31, 1893.

GENERAL FUND.

	£	s.	d.
W. M. Conway, in aid of an Expedition to the Karakoram Mountains	100	0	0
L. Oertling, for Repair of Apparatus belonging to the Government Grant Committee.	6	10	6
Secretary of the Colour-Vision Committee, towards the Expense of the Committee's Researches.	60	0	0
H. A. Miers, for an Investigation of the Irregularities in the Angles of Crystals in Isomorphous Mixtures ...	40	0	0
R. P. Chattock, for an Investigation of the Absolute Measurement of the Charge conveyed by the Ions of an Electrolyte	10	0	0
J. Joly, for a Research on the Specific Heats of Gases at Constant Volume.	15	0	0
Prof. C. G. Knott, for an Investigation of the Effect of Magnetism upon the Internal Volume of Iron, Steel, and Nickel Tubes of various Bores	50	0	0
Prof. Poynting, for aid in Experimenting on the Construction of a Portable Differential Gravity Meter with Temperature Compensation.	30	0	0
Prof. W. Ramsay, for a Verification of the Law $pv = RT$, for Gases under Low Pressures (£25); and for an Investigation of the Capillary Constants of Liquids up to their Critical Points (£25).	50	0	0
Profs. Rücker and Thorpe, for the Completion of the Magnetic Survey of the British Isles.	650	0	0
Prof. Worthington, for Measurement of the Relations between Tensile Stress and Strain in Liquids at Different Temperatures	50	0	0
H. L. Callendar, for a Comparison of Platinum and Air Thermometers.	80	0	0

Carried forward.......... £1,141 10 6

	£	s.	d.
Brought forward	1,141	10	6
Prof. W. N. Hartley, for the Investigation of High Temperature Flame-spectra by the aid of Photography	100	0	0
Prof. Dewar, for a Research on the Physical Properties of Liquid and Solid Oxygen, Nitrogen, and Air	300	0	0
Prof. Dunstan, for Further Research on the Action of Alkalis on Nitro-compounds, &c....................	50	0	0
Research Committee, Pharmaceutical Society (per Prof. Dunstan), for Continuation of the Chemical Investigation of the Aconite Alkaloids (£100), and the Isolation and Chemical Examination of the Substance to which Croton Oil owes its vesicating property.. ...	150	0	0
J. A. Gardner, for Further Investigation of the Terpenes and more especially of Camphene	30	0	0
W. P. Wynne, for Payment of an Assistant to aid in a Research on the Action of Sulphuric Acid on Quinoline...................................	50	0	0
Prof. W. H. Perkin, jun., for Experiments on the Constitution of Camphoric and Camphoronic Acid and other Allied Acids	75	0	0
W. A. Shenstone, for Further Investigation (1) of the Action of the Silent Discharge in Oxygen and other Gases; (2) of the Production of Haloid Salts from Pure Materials................................	20	0	0
Drs. G. Dyson and A. Harden, for the Investigation of the Combination of Carbon Monoxide and Chlorine under the Influence of Light	10	0	0
R. Irvine, for Continuation of Investigations on Oceanographic Chemistry......................	100	0	0
W. T. Thiselton Dyer, to enable the Botanist accompanying the Sierra Leone Delimitation Commission to prolong his Expedition if necessary	100	0	0
J. G. Grenfell, for a Research on the Diatomaceæ with Protoplasmic Pseudopodia	25	0	0
G. Brebner, for one year's study at Cumbrae of the Life-history of Local Species of certain Genera of the Florideæ.................................	100	0	0
Prof. W. C. Williamson, for a Research on the Microscopic Structure of Coal and Carboniferous Plants	25	0	0
H. Wager, for a Research on the Minute Structure of the Cells of Fungi and Bacteria, and the Processes of Fertilisation in the Fungi....................	38	5	0
Carried forward........	£2,314	15	6

	£	s.	d.
Brought forward	2,314	15	6
Dr. Sclater, for Expense of sending an Ornithologist to Uruguay to collect Birds with a view to publication of a Monograph	100	0	0
West India Committee (per G. Murray), for further aid in collecting and describing Fauna and Flora in the less known West Indian Islands.....................	100	0	0
Sandwich Islands Committee (per D. Sharp), for the Investigation of the Fauna of the Sandwich Islands ...	200	0	0
Prof. Weldon, for the Measurement of various Organs in Statistically Large Examples of various Local Races of Common Animals	50	0	0
E. J. Bles, for a Research on the Marine Floating Organisms of the British Seas, and the Effect of Changes in Environment on their Distribution.................	100	0	0
T. Scott, for Completion of the Study and Description of the Marine Entomostraca obtained by Mr. John Rattray in 1886	20	0	0
F. E. Beddard, for Procuring Oligochœta from East Tropical Africa...................................	100	0	0
C. D. Marshall, for a Research on the Transmission of Nerve Impulses from the Cortex Cerebri by the Channels of the Spinal Cord.........................	50	0	0
W. G. Spencer, for a Research on the Central Innervation of the Respiration and Circulation in connexion with Increased Intracranial Pressure................	100	0	0
Dr. Starling, for an Investigation of some points in the Physiology of the Mammalian Heart.............	40	0	0
Dr. J. R. Bradford, for the further Investigation of the Disturbance of Nutrition producible in Dogs by Removal of Portions of the Kidney.........................	150	0	0
J. H. Parsons, for a Research on the Functions of the Thyroid Gland	20	0	0
Dr. W. H. Thompson, for a Research on the Course and Cerebral Connexions of Cranial Nerves	40	0	0
Prof. P. F. Frankland, for Continuation of Researches on the Chemical Changes brought about by Specific Micro-organisms......................................	75	0	0
C. S. Sherrington, for the Examination of the Actions and the Topography of Reflex and Automatic Centres in the Lower Half of the Spinal Cord.................	75	0	0
Carried forward	£3,534	15	6

	£	*s.*	*d.*
Brought forward	3,534	15	6
Dr. Gaskell, for a Research to be carried out by Mr. F. Edgeworth on the Distribution and Function of the Large Fibres in the Sympathetic System	30	0	0
Dr. Copeman, for Researches on the Bacteriology of Vaccine Lymph	60	0	0
	£3,624	15	6

RESERVE FUND.

	£	*s.*	*d.*
Eclipse (1893) Committee, for the Expenses of the Eclipse Expedition.......................	600	0	0
Treasurer, Royal Society, for an Inquiry (in conjunction with the London County Council) on the Vitality of Microscopic Pathogenic Organisms in Large Bodies of Water	500	0	0
	£1,100	0	0

GENERAL FUND.

Dr.						Cr.		
	£	*s.*	*d.*			£	*s.*	*d.*
To Balance, November 30, 1891 .	373	14	3		By Appropriations, as above	3,624	15	6
,, Grant from Treasury	4,000	0	0		,, Salaries, Printing, Postage, Advertising, and other Administrative Expenses	142	9	2
., Repayments	204	5	6		,, Balance, Mar. 31, 1893	845	7	9
,, Interest on Deposit........	34	12	8					
	£4,612	12	5			£4,612	12	5

RESERVE FUND.

Dr.						Cr.		
	£	*s.*	*d.*			£	*s.*	*d.*
To placed to Reserve Fund in 1890	1,576	12	1		By Appropriations, as above	1,100	0	0
,, ditto in 1891	423	7	11		,, Balance, Mar. 31, 1893	900	0	0
	£2,000	0	0			£2,000	0	0

Report of the Kew Committee for the Year ending December 31, 1892.

The operations of The Kew Observatory, in the Old Deer Park, Richmond, Surrey, are controlled by the Kew Committee, which is constituted as follows :

Mr. F. Galton, *Chairman.*

Captain W. de W. Abney, C.B., R.E.

Prof. W. G. Adams.

Captain E. W. Creak, R.N.

Prof. G. C. Foster.

Admiral Sir G. H. Richards, K.C.B.

The Earl of Rosse, K.P.

Prof. A. W. Rücker.

Mr. R. H. Scott.

Lieutenant-General R. Strachey, C.S.I.

General J. T. Walker, C.B.

Captain W. J. L. Wharton, R.N.

The serious illness of Mr. Whipple has prevented his performing the duties of Superintendent during the last half-year. During this period the work of the Observatory was very satisfactorily carried out by Mr. Baker, the Chief Assistant, and the Committee are of opinion that his services should be specially recorded, and they are glad to state that the routine work of the Observatory has in no way suffered in these circumstances.

The work at the Observatory may be considered under the following heads :—

1st. Magnetic observations.
2nd. Meteorological observations.
3rd. Solar observations.
4th. Experimental, in connexion with any of the above departments.
5th. Verification of instruments.
6th. Rating of Watches and Marine Chronometers.
7th. Miscellaneous.

I. MAGNETIC OBSERVATIONS.

There have been no changes introduced in the magnetographs during the past year, but during the erection of the additional story to the west wing of the Observatory the self-recording instruments were at times disturbed by the building operations. Fortunately the indications of the instruments were seriously affected by these causes *on one of the* " quiet days " only, and that day has been omitted in *calculating the* monthly mean.

The building in which the absolute observations are made is sufficiently remote (about 100 yards) from the main building to be quite unaffected by these sources of magnetic disturbance.

The photographed magnetic curves representing Declination, Horizontal Force, and Vertical Force variations have been secured uninterruptedly throughout the year, and, as usual, the scale values of all the instruments were determined in January last.

The following values of the ordinates of the different photographic curves were then found:—

Declinometer: 1 inch = 0° 22′·04.   1 cm. = 0° 8″·7.

Bifilar, January 5, 1892, for 1 inch δH = 0·0280 foot grain unit.
$\quad$ „ 1 cm. „ = 0·00050 C.G.S. unit.
Balance, January 7, 1892, for 1 inch δV = 0·0287 foot grain unit.
$\quad$ „ 1 cm. „ = 0·00052 C.G.S. unit.

In the case of the Vertical Force magnetometer, it was found necessary to readjust the instrument; at the same time its sensibility was slightly altered, after which the scale value was again determined with the following result:—

Balance, January 15, 1892, for 1 inch δV = 0·0277 foot grain unit.
$\quad$ „ 1 cm. „ = 0·00050 C.G.S. unit.

The distance between the dots of light upon the Horizontal Force cylinder having become too large for satisfactory registration, the trace dot was brought nearer to the zero dot on August 6.

The principal magnetic disturbances were recorded on the following dates, viz. :—February 13—14, March 6 and 12, April 26, May 18, July 16—17, and August 12.

The most marked disturbance, however, was that which commenced on February 13 at 5.34 A.M., and lasted until the afternoon of the 14th.

The oscillations were of a more extended and violent character than any which have been recorded during the last ten years.

From the insufficient range of the scale, the magnetometers did not record the complete extent of the vibrations to which the needles were subjected, nor could the entire change of force be secured in the field of the instrument.

The limits, however, clearly recorded, were 1° 40′ of declination, from 0·1755 to 0·1835 of horizontal force, and from 0·4350 to 0·4425 units of vertical force expressed in C.G.S. measure in absolute force.

The following table exhibits the absolute hourly values of Declination (Inclination calculated from the Horizontal and Vertical Forces), the Horizontal Force and Vertical Force having both been corrected for *temperature* for February 13, 14, and 15, 1892 :—

2 K 2

Hour.	Declination.			Inclination.		
	Feb. 13.	Feb. 14.	Feb. 15.	Feb. 13.	Feb. 14.	Feb. 15.
1 A.M..	17° 34·7	18° 31·7	17° 39·5	67° 31·2	° ..	67° 33·6
2 „ ..	35·9	17 56·7	38·4	30·9	67 43·7	33·5
3 „ ..	37·7	27·7	38·7	30·5	36·2	33·2
4 „ ..	38·9	38·2	38·7	30·2	40·0	33·0
5 „ ..	38·9	52·7	40·6	29·8	42·5	32·5
6 „ ..	39·7	40·6	38·9	29·0	38·4	32·1
7 „ ..	33·7	38·7	40·7	29·9	38·2	32·4
8 „ ..	32·1	35·2	38·7	29·1	37·7	34·0
9 „ ..	35·1	34·7	36·7	28·3	36·3	32·9
10 „ ..	24·0	34·5	37·7	30·6	37·9	34·2
11 „ ..	47·7	36·2	39·2	33·5	37·2	33·1
Noon...	36·7	38·2	40·7	38·0	36·9	33·6
1 P.M...	21·7	41·7	42·7	33·4	34·7	32·4
2 „ ..	28·7	42·4	40·7	35·6	33·7	32·9
3 „ ..	37·8	41·2	38·7	36·3	34·9	29·6
4 „ ..	33·7	41·2	37·8	30·9 ·	36·5	32·4
5 „ ..	17·8	40·7	37·3	..	35·7	33·6
6 „ ..	16·7	40·2	36·6	34·3	34·7	32·6
7 „ ..	45·5	39·9	42·7	32·2	34·8	35·2
8 „ ..	50·7	39·0	39·7	36·1	34·5	33·5
9 „ ..	42·1	39·5	39·7	38·9	34·5	31·5
10 „ ..	58·7	37·7	39·7	58·7	34·4	31·6
11 „ ..	17 37·7	36·9	35·7	40·8	34·3	33·3
Mid ...	18 7·5	41·1	36·7	30·4	34·4	32·9

Hour.	Horizontal force.			Vertical force.		
	Feb. 13.	Feb. 14.	Feb. 15.	Feb. 13.	Feb. 14.	Feb. 15.
1 A.M. .	0·18184	0·17622	0·18144	0·43943	trace off sheet	0·43934
2 „ ..	0·18188	0·17872	0·18147	0·43914	0·43638	0·43938
3 „ ..	0·18194	0·18066	0·18151	0·43943	0·43838	0·43936
4 „ ..	0·18198	0·18038	0·18155	0·43942	0·43908	0·43938
5 „ ..	0·18204	0·17977	0·18161	0·43941	0·43851	0·43936
6 „ ..	0·18197	0·18070	0·18167	0·43927	0·43928	0·43936
7 „ ..	0·18192	0·18087	0·18162	0·43914	0·43961	0·43933
8 „ ..	0·18206	0·18097	0·18137	0·43922	0·43969	0·43933
9 „ ..	0·18225	0·18121	0·18156	0·43938	0·43976	0·43936
10 „ ..	0·18182	0·18099	0·18135	0·43917	0·43978	0·43933
11 „ ..	0·18145	0·18117	0·18151	0·43934	0·44000	0·43931
Noon ...	0·18086	0·18119	0·18144	0·43952	0·43993	0·43933
1 P.M...	0·18185	0·18152	0·18170	0·44024	0·43993	0·43952
2 „ ..	0·18184	0·18174	0·18167	0·44104	0·44011	0·43962
3 „ ..	0·18202	0·18175	0·18222	0·44174	0·44053	0·43978
4 „ ..	0·18312	0·18137	0·18182	0·44244	0·44020	0·43981
5 „ ..	0·18298	0·18142	0·18165	trace off sheet	0·44003	0·43983
6 „ ..	0·18248	0·18152	0·18172	0·44212	0·43991	0·43966
7 „ ..	0·18261	0·18146	0·18125	0·44164	0·43983	0·43945
8 „ ..	0·18161	0·18150	0·18154	0·44064	0·43981	0·43954
9 „ ..	0·18107	0·18149	0·18182	0·44038	0·43978	0·43951
10 „ ..	0·17773	0·18150	0·18172	0·43944	0·43978	0·43931
11 „ ..	0·18061	0·18149	0·18145	0·43994	0·43971	0·43925
Mid	0·18134	0·18146	0·18156	0·43794	0·43969	0·43936

The following are the principal results of the magnetic elements for the year 1892 :—

Mean Westerly Declination........	17° 36'·7
Mean Horizontal Force............	0·18202 C.G.S. unit.
Mean Inclination	67° 29'·4
Mean Vertical Force........	0·43919 C.G.S. unit.

Additional observations of the Horizontal Force, Inclination, and Declination have been made each month with the absolute instruments, for the purpose of determining with greater precision the zero values of the magnetograph curves.

Information on matters relating to various magnetic data has been supplied to Lord Kelvin, P.R.S., Professor Rücker, Dr. Neumayer, Captain Schück, and Dr. Atkinson.

Lieutenant C. E. Monro, of H.M.S. "Penguin," visited the Observatory from November 11 to 22, in order to gain a knowledge of the use of the unifilar magnetometer and the method of observing and reducing the observations.

Mr. E. Kitto, Superintendent of the Falmouth Observatory, again spent a fortnight at Kew, in the spring, preparing for the reduction, upon the International scheme, of the magnetic observations made at that Observatory.

From time to time Messrs. Gray and Watson have visited the Observatory for the purpose of taking a series of absolute magnetic observations with the instruments which have been employed under the direction of Professors Rücker and Thorpe in their magnetic survey of the British Isles.

A glass scale graduated in millimetres for measuring magnetic curves was constructed for Professor W. G. Adams.

A number of Thomson compass deflectors by J. White, of Glasgow, have been examined, the examination being conducted at the Observatory by Mr. Baker, acting upon suggestions made by Captain Creak.

II. Meteorological Observations.

The several self-recording instruments for the continuous registration respectively of Atmospheric Pressure, Temperature of Air and Wet-bulb, Wind (direction and velocity), Bright Sunshine, and Rain, have been maintained in regular operation throughout the year, and the standard eye observations for the control of the automatic records duly registered.

The tabulations of the meteorological traces have been regularly made, and *these*, as well as copies of the eye observations, with

notes of weather, cloud, and sunshine, have been transmitted, as usual, to the Meteorological Office.

With the sanction of the Meteorological Council, data have been supplied to the Council of the Royal Meteorological Society, the editor of ' Symons's Monthly Meteorological Magazine,' Dr. Rowland, the Institute of Mining Engineers, and others.

Detailed information of all thunderstorms observed in the neighbourhood during the year has been forwarded to the Royal Meteorological Society, as usual.

At the request of the Meteorological Council, experiments have been for some months in progress upon the spare Beckley Rain Gauge with Willesden prepared paper and aniline ink, with the view of determining its adaptability for use with that instrument, as a substitute for the paper hitherto used, which has been found to deteriorate on keeping.

Daily trials were carried out, and the results showed a marked improvement upon those previously obtained. It was found impossible, however, to entirely prevent the lengthening of the papers during very damp weather, although the sheets were soaked and coated with various varnishes, &c. Experiments are still in progress on this subject.

Various suggestions for a supplemental record of the number of discharges made by the Beckley Rain Gauge during heavy rainfalls have been under consideration, but nothing definite has, up to the present, been decided upon.

Anemograph.—A new pricker was fitted to this instrument in June, the old one having become bent and loose in its fitting.

Sunshine Records.—As it was found that the scaffolding erected during the extension of the west wing interfered with the registration of bright sunshine by the recorder after 6 P.M., a spare instrument was obtained on loan from the Meteorological Office, and fitted up on the staging above the sun room, in order to prevent any possible loss of record, and was in use from August 2 to September 10, the scaffolding being removed on the latter date.

Alterations in Observatory.—To facilitate photographic operations, and to keep the thermograph free from disturbance, &c., the curtains heretofore used in the room have been removed, and a wooden partition with two doors erected, which has been found a great improvement. At the same time, arrangements were made so as to render the room available for the registering portion of the electrograph, and the two instruments are now conveniently placed side by side.

Inspections.—At the commencement of March Mr. Whipple visited the Valencia Observatory, and after dismounting the whole of the meteorological instruments, conveyed them to the new building

constructed for the purpose at Westwood House, Cahirciveen, the new Valencia Observatory. The removal was successfully accomplished with only one breakage, that of the wet-bulb reference thermometer. Before leaving, Mr. Whipple made careful determinations of the level of the barometer at the new station, the heights of anemometer cups, rain gauges, and thermometers above ground, re-determined index errors, &c.

At the request of the Meteorological Council, Mr. Baker visited the Glasgow Observatory in April, taking with him three new thermograph tubes and two Kew standard thermometers of reference, in order to replace instruments which had been maliciously broken.

Electrograph.—This instrument was kept in action until the end of July, when it was dismounted to prevent possible damage during the building operations connected with the extension of the west wing of the Observatory. The scale value was determined by direct comparison with the portable electrometer, No. 53, early in May and at the end of June. On the completion of the building, the instrument being in a somewhat inconvenient spot, rendering dislocation possible, it was decided to remove it to a safer position, which was rendered accessible by the alterations to the thermograph room. The water reservoir, however, was not moved, as this might perhaps have interfered with the continuity of the records, and it is intended to commence again the regular records with the beginning of 1893.

III. Solar Observations.

Sun-spots.—Sketches of Sun-spots have been made on 178 days, and the groups numbered after Schwabe's method.

On no occasion during the year, when observations have been taken, has the Sun's surface been found free from spots, and the number of new groups enumerated has largely increased.

Time Signals.—These have been regularly received from Greenwich through the G.P.O., with the exception of a few days, on which occasions supplementary signals were transmitted at later hours, and a list of time corrections sent when required.

Transit Observation.—Occasional solar and sidereal transits have been observed as checks upon the Greenwich signalled times.

Violle's Actinometer.—With regard to these instruments, the only observations made during the past year were experiments to determine the rate of cooling both of the spheres and the thermometers used in connection with them. The weight of each sphere also was determined when filled with water. The results were forwarded to Mr. H. F. Blanford, F.R.S., who had undertaken to investigate the subject for the Solar Physics Committee.

IV. EXPERIMENTAL WORK.

In accordance with the request of Mr. Ellery, the Government Astronomer at Melbourne, the Indian pendulum apparatus, having been thoroughly overhauled since its return from the Royal Observatory, Greenwich, to Kew, was carefully packed and shipped to Melbourne, for use in the Gravity Survey now being undertaken by the Australian authorities.

The packing and shipping were conducted under the direction of General Walker, who prepared a detailed statement of the necessary instructions to be followed by the observers.

The Richard thermograph, procured for use with the apparatus, was also carefully packed and sent to Melbourne. Notice has been received of the arrival in the Colony of the apparatus.

Cloud Photographs.—Operations connected with cloud photography have been suspended during the past year. At the request of the Meteorological Office, certain cloud negatives taken in 1891, with their reductions, were forwarded to them for examination, as well as the apparatus used in the reduction of their heights and velocities.

Fog and Mist.—With the view of ensuring greater uniformity in observations of these phenomena, at the suggestion of Mr. R. H. Scott, a list of twenty-four well-known objects in the neighbourhood of the Observatory has been prepared, at distances varying from 9 to 3850 yards. Since May, the most distant of the objects visible at each observation hour between sunrise and sunset has been noted. Up to the present the most dense fog recorded was when an object at 20 yards distance was obscured.

Further experiments were made at the beginning of the year with Munro's sight indicating anemometer, but the variation of viscosity of the oil at low temperatures has caused some difficulty in determining the scale value of the instrument, which has been returned to the maker.

V. VERIFICATION OF INSTRUMENTS.

The following instruments have been purchased on commission and their constants determined:—

1 pair of dip needles, for the Meteorological Institute, Copenhagen.

1 pair of dip needles for the Imperial and Royal Austro-Hungarian Embassy, London.

1 Clifton electrometer, water dropping collector and insulators, also a battery of 60 chloride of silver cells and a dip needle for the *Royal Alfred Observatory*, Mauritius.

A set of 24 thermometers for the Observatory, Hong Kong.

The total number of other instruments compared during the year was as follows:—

	Number tested in the year ending December 31, 1892.	Number tested during the fourteen months ending December 31, 1891.
Air-meters	9	7
Anemometers	4	19
Aneroids	74	72
Artificial horizons	22	10
Barometers, Marine	74	111
„ Standard	61	57
„ Station	18	39
Binoculars	168	470
Compasses	28	22
Deflectors	20	0
Hydrometers	395	224
Inclinometers	1	3
Photographic Lenses	18	19
Magnets	1	2
Navy Telescopes	487	374
Rain Gauges	9	17
Rain Measures	13	39
Sextants	463	428
„ Shades	52	7
Sunshine Recorders	1	1
Theodolites	6	5
Thermometers, Arctic	50	133
„ A vitreous or Immisch's	71	231
„ Chemical	44	108
„ Clinical	16,850	15,692
„ Deep sea	31	58
„ Meteorological	1,875	2,289
„ Mountain	17	26
„ Solar radiation	1	1
„ Standards	79	62
Unifilars	1	3
Vertical Force Instruments	5	0
Total	20,948	20,529

Duplicate copies of corrections have been supplied in 78 cases.

The number of instruments rejected on account of excessive error, or for other reasons, was as follows:—

Thermometers, clinical 32
　　　　　　　„　　　ordinary meteorological.......... 13
　　Sextants 83
　　Telescopes 90
　　Various 21

3 Standard Thermometers have been supplied during the year.

There were at the end of the year in the Observatory undergoing verification, 12 Barometers, 202 Thermometers, 8 Hydrometers, 13 Sextants, 21 Telescopes, and 1 Anemometer.

At the request of Captain Tyler, R.E., Inspecting Officer of the R.E. Division, Royal Dockyard, Woolwich, a batch of 72 telescopes for the use of the officers of the field artillery has been examined.

VI. RATING OF WATCHES.

1044 watches have been sent for examination during the year, as contrasted with 709 during the fourteen months comprised in last report. They were entered for the following classes :—

For class A, 414; class B, 403; class C, 221; and 6 for the subsidiary trial. Of these 192 failed from various causes to gain any certificate; 214 were awarded class C certificates, 377 class B, and 256 class A; of the latter, 22 obtained the highest form of certificate, class A, *especially good;* and 5 of the 6 passed the second test.

In the Appendix will be found statements giving the results of trial of the 22 watches which gained the highest number of marks during the year. The first place was taken by Messrs. Baume and Co., London, with a keyless, going-barrel, chronometer-watch, No. 103,018, with the " tourbillon " escapement, which obtained the high total of 91·9 marks out of a maximum of 100; this is the highest value yet awarded.

The best performance of *lever* watches during the year was that of No. 13,400 by Fridlander, Coventry, which gained 86 marks.

There has been a marked increase in the number of watches sent for the B and C trials, and the use of these tests for lower-graded movements appears, judging by the demand, to be steadily growing in favour.

Non-Magnetic Watches.—Several watches thus designated have been examined during the year, both as to their ordinary time-keeping and also to their non-magnetic properties. The trial is rigorous, the movement being tested in an intense magnetic field, both in vertical and horizontal positions, and gradually approached to and removed from the coil, whilst its behaviour is critically watched, and its *subsequent daily rate* noted. Should any alterations of its normal *performance* occur, the watch receives no certificate.

Marine Chronometers.—During the year, 9 class A and 9 class B certificates have been issued with chronometers which had undergone the tests, as described in last report; one movement failed to pass the trials.

VII. MISCELLANEOUS.

Lens Testing.—A detailed account of the apparatus and methods employed in the examination of lenses has been completed by Major Darwin, and presented to the Royal Society. The paper is being printed *in extenso* by several photographic journals. Major Darwin also read a paper on this subject before the Photographic Society of Great Britain, the apparatus being illustrated by means of lantern slides. The Lens Testing Camera was shown at the Soirée of the Royal Society, in May.

A loan of twelve lenses, all known to be of good quality, has been obtained from the Royal Engineering School at Chatham, by the kind permission of the Commandant. These have been subjected to a very detailed examination, the results of which will be considered as standards of reference for other lenses sent here for certification.

Experiments are in progress in the endeavour to find an object more suitable for the "definition" test than the one now in use.

Library.—During the year the library has received as presents the publications of—

37 Scientific Societies and Institutions of Great Britain and Ireland, and

106 Foreign and Colonial Scientific Establishments, as well as of numerous private individuals.

The preparation of the card catalogue of the Library is still continued, but confined only to such publications as relate to Meteorology, Terrestrial Magnetism, and the other work of the Observatory.

Extension of the Building.—The Chief Commissioner of Works and Public Buildings having granted permission for the Committee to undertake the erection of the additional story to the west wing of the Observatory, as mentioned in last year's Report, and having instructed Mr. Lessels, surveyor to the Board, to prepare the necessary drawings, plans, &c., tenders were invited from the principal local builders for the work. That of Messrs. J. Dorey and Co., of Brentford, for £540, was accepted, and operations were commenced on July 15. They have now been completed, under the superintendence of Mr. Chart, H.M. Commissioners' Clerk of Works for the Hampton Court and Kew District, and Mr. Allen, his assistant.

The cost of the operations being a heavy charge on the funds at the present disposal of the Committee, they made application to the *Royal Society* for a loan of £400, which was liberally granted.

During the building alterations the thermometer testing was carried on in the experimental magnetic house.

Water Supply.—Applications having been made to Her Majesty's Office of Works for the provision of a direct water supply, available for the protection of the building in the case of fire and other purposes, arrangements were made with the Water Committee of the Richmond Corporation for the laying of a branch main along the roadway leading from Clarence Street, Richmond, to the Observatory, and H.M. Office of Works contributed a moiety of the cost, viz., £74 10s.

Paper.—Prepared photographic paper has been procured, and supplied to the Observatories at Aberdeen, Falmouth, Fort William, Lisbon, Mauritius, Oxford, Stonyhurst, Valencia, Hong Kong, Toronto, as well as to the Meteorological Office for Batavia.

Anemograph sheets have been sent to Mauritius, and blank forms for entry of magnetic observations to Professor Rücker and Dr. Meldrum.

Exhibition of Instruments.—Various instruments were shown by the Committee at the thirteenth annual exhibition of the Royal Meteorological Society.

Workshop.—The machine tools procured for the use of the Kew Observatory by grants from the Government Grant Fund or the Donation Fund have been duly kept in order.

House, Grounds and Footpath.—These have all been kept as usual during the year.

PERSONAL ESTABLISHMENT.

The staff employed is as follows :—

    G. M. Whipple, B.Sc., Superintendent.
    T. W. Baker, Chief Assistant.
    H. McLaughlin, Accounts and Library.
    E. G. Constable, Observations and Rating.
    W. Hugo, Verification Department.
    J. Foster   „   „
    T. Gunter   „   „
    W. J. Boxall   „   „
    E. Dagwell, and seven other Assistants.

                (Signed) FRANCIS GALTON,
                                Chairman.

Comparison of Expenditure (excluding Commissions) for the twelve months ending December 31st, 1891, and December 31st, 1892.

Net expenditure.	1891.			1892.			Increase.			Decrease.		
	£	s.	d.	£	s.	d.	£	s.	d.	£	s.	d.
Administration—												
Superintendent......	400	0	0	400	0	0						
Office	207	1	6	200	3	0	..			6	18	6
Rent, fuel, and lighting	53	7	7	58	15	10	5	8	3			
Attendance and contingencies	191	13	9	184	12	10	..			7	0	11
Normal Observatory—												
Salaries	284	4	8	296	12	0	12	7	4			
Incidental expenses..	49	5	5	31	14	11	..			17	10	6
Researches—												
Salaries	221	6	0	223	5	0	1	19	0			
Incidental expenses..	28	1	8	2	11	0	..			25	10	8
Tests—												
Salaries	876	14	6	858	17	7	..			17	16	11
Incidental expenses..	260	2	10	183	15	2	..			76	7	8
Extension of Premises—												
West wing..........	..			500	0	0	500	0	0			
Water main	..			156	10	0	156	10	0			
							676	4	7	151	5	2
							151	5	2			
	2,571	17	11	3,096	17	4	524	19	5			

Kew Observatory. Account of Receipts and Payments for the year ending December 31st, 1892.

Dr.   RECEIPTS.

		£ s. d.
To Balance from Year 1891		380 6 10
Royal Society:—	£ s. d.	
(Gassiot Trust)	487 10 0	
(Loan for Building)	400 0 0	
		887 10 0
Meteorological Council:—		
Allowance	400 0 0	
Postages, &c.	3 3 11	
		403 3 11
Researches		2 4 6
Tests:—		
Verifications	1214 18 4	
Rating	633 18 8	
Lenses	7 17 9	
		1856 14 9
H.M.'s Office of Works, Share of Water Main		74 10 0
Commissions executed for Colonial and Foreign Institutions, &c.		243 18 4
		£3848 8 4

PAYMENTS.   Cr.

	£ s. d.	£ s. d.
By Administration:—		
Superintendent	400 0 0	
Salaries, Wages, &c.	200 8 0	
Rent, Fuel, and Lighting	58 15 10	
Attendance, Cleaning, Repairs, Insurance, &c.	184 12 10	
		843 11 8
Normal Observatory:—		
Salaries—Observations, Tabulations, &c.	296 12 0	
Incidental Expenses—Instruments, Postages, &c.	31 14 11	
		328 6 11
Researches:—		
Salaries—Observations, Reductions, &c.	223 5 0	
Incidental Expenses—Instruments, Postages, &c.	2 11 0	
		225 16 0
Tests:—		
Salaries	858 17 7	
Incidental Expenses—Instruments, Porterage, Printing, &c.	183 15 2	
		1042 12 9
Extension of Premises:—		
West Wing	500 0 0	
Water Main from Richmond to the Observatory	156 10 0	
		656 10 0
Commissions executed for Colonial and Foreign Institutions, &c.		222 8 3
Balance:—		
Cash at Bank of England	327 18 10	
„ London and County Bank, Richmond	112 10 0	
„ Observatory (for Banking)	76 13 3	
„ „ (Petty Cash)	12 0 8	
	88 13 11	
		529 2 9
		£3848 8 4

February 6, 1893.   Examined and compared with the vouchers, and found correct.   (Signed)   W. GRYLLS ADAMS, *Auditor.*

ESTIMATED ASSETS.

	£	s.	d.
By Balance as per Statement	529	2	9
Payments:—	£ s. d.		
Meteorological Council—Allowance, Postage, &c.	109 14 11		
Test Fees	341 11 2		
Commission	34 16 8		
	486	12	9
Stock:—			
Blank Forms and Certificates	48 16 9		
Standard Thermometers	87 16 0		
	136	12	9
	£1152	8	3

ESTIMATED LIABILITIES.

	£	s.	d.
To Administration accounts—Gas, Repairs, and Contingencies	40	1	2
Observatory accounts—A.G.B. Paper, Chemicals, &c.	35	17	8
Tests accounts—Fittings, Printing, Stationery, &c.	23	6	0
Unspent Balance of Pendulum Account	117	1	7
Royal Society (Loan)	400	0	0
Dorey & Co.—Balance for Building	40	0	0
Commissions	43	16	9
General Balance	452	5	6
	£1152	8	3

February 27, 1893.

(Signed)   T. W. BAKER,
Chief Assistant.

List of Instruments, Apparatus, &c., the Property of the Kew Committee, at the present date out of the custody of the Superintendent, on Loan.

To whom lent.	Articles.	Date of loan.
G. J. Symons, F.R.S.	Portable Transit Instrument...............	1869
The Science and Art Department, South Kensington.	The articles specified in the list in the Annual Report for 1876, with the exception of the Photo-Heliograph, Dip-Circle, Unifilar, and Hodgkinson's Actinometer.	1876
R. J. Ellery, F.R.S..	Pendulum Apparatus, complete, with Richard Thermograph.	1892
Professor W. Grylls Adams, F.R.S.	Unifilar Magnetometer, by Jones, No. 101, complete.	1883
	Pair 9-inch Dip-Needles with Bar Magnets ...	1887
Professor O.J. Lodge, F.R.S.	Unifilar Magnetometer, by Jones, No. 106, complete. Barrow Dip-Circle, No. 23, with two Needles, and Magnetizing Bars. Tripod Stand.	1883
Captain W. de W. Abney, F.R.S.	Mason's Hygrometer, by Jones	1885
Prof. T. E. Thorpe, F.R.S.	Tripod Stand........................	1886
Lord Rayleigh, F.R.S.	Standard Barometer (Adie, No. 655)	1885
Mr. C. Eldridge	Chain Anemometer	1890

APPENDIX I.

MAGNETICAL OBSERVATIONS,

Made at the Kew Observatory, Richmond, Lat. 51° 28′ 6″ N. and Long. $0^h 1^m 15^s\cdot 1$ W., height 34 feet above mean sea-level, for the year 1892.

The results given in the following tables are deduced from the magnetograph curves which have been standardised by observations of deflection and vibration. These were made with the Collimator Magnet K.C. I. and the Declinometer Magnet marked K.O. 90 in the 9-inch Unifilar Magnetometer by Jones.

The Inclination was observed with the Inclinometer by Barrow, No. 33, and needles 1 and 2, which are $3\frac{1}{2}$ inches in length.

The Declination and Force values given in Tables I to VIII are prepared in accordance with the suggestions made in the fifth report of the Committee of the British Association on comparing and reducing Magnetic Observations.

The following is a list of the days during the year 1892 which were selected by the Astronomer Royal, as suitable for the determination of the magnetic diurnal variations, and which have been employed in the preparation of the magnetic tables.

January	2, 9, 20, 22, 30.
February	3, 8, 17, 18, 22.
March	10, 14, 17, 18, 23.
April..............	5, 6, 17, 20, 22.
May	12, 13, 15, 23, 26.
June	8, 9, 12, 14, 15.
July	5, 6, 8, 20, 23.
August	11, 14, 15, 19, 30.
September	4, 5, 9, 12, 25.
October............	9, 17, 23, 26, 28.
November..........	8, 11, 12, 16, 27.
December..........	3, 9, 18, 26 27.

Table I.—Hourly Means of Declinati

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
(17° +) West.					Winter.						
1892. Months.	′	′	′	′	′	′	′	′	′	′	′
Jan. ..	38·0	37·9	37·6	37·4	37·5	37·6	37·8	38·0	37·6	37·9	39·5
Feb. ..	37·6	37·4	37·7	37·5	37·9	37·3	38·3	38·2	38·3	38·6	40·0
March .	37·1	37·3	37·4	37·8	37·4	37·8	37·5	36·0	35·2	35·6	37·5
Oct. ..	32·3	32·7	32·9	33·0	32·8	33·1	32·6	32·4	31·3	31·0	32·6
Nov. ..	32·9	33·2	33·2	33·2	33·1	32·7	32·9	32·9	32·6	32·3	32·7
Dec. ..	32·2	32·5	32·8	32·9	33·0	33·2	32·6	32·8	32·7	33·1	34·4
Mean .	35·0	35·2	35·3	35·2	35·3	35·3	35·3	35·1	34·6	34·8	36·1
					Summer.						
	′	′	′	′	′	′	′	′	′	′	′
April ..	36·4	36·2	35·7	35·7	35·4	34·8	33·9	32·6	31·7	31·9	34·6
May ..	37·0	36·9	36·7	36·2	35·3	33·7	32·5	31·8	32·6	34·4	37·5
June ..	36·1	35·8	35·8	35·7	34·3	32·3	31·0	30·6	31·1	33·0	35·9
July ..	37·1	36·8	35·7	35·4	34·5	32·5	31·3	31·4	31·2	32·3	34·5
Aug. ..	35·6	35·4	35·5	34·6	34·2	33·1	31·6	30·9	31·2	33·1	36·7
Sept. ..	35·8	35·1	34·8	34·5	34·1	33·6	32·7	32·4	32·6	34·6	38·2
Mean .	36·3	36·0	35·7	35·4	34·6	33·3	32·2	31·6	31·7	33·2	36·2

Table II.—Solar Diurnal Range of the

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
					Summer Mean.						
	′	′	′	′	′	′	′	′	′	′	′
	−0·5	−0·8	−1·1	−1·4	−2·2	−3·5	−4·6	−5·2	−5·1	−3·6	−0·6
					Winter Mean.						
	′	′	′	′	′	′	′	′	′	′	′
	−1·7	−1·5	−1·4	−1·5	−1·4	−1·4	−1·4	−1·6	−2·1	−1·9	−0·6
					Annual Mean.						
	′	′	′	′	′	′	′	′	′	′	′
	−1·1	−1·2	−1·3	−1·5	−1·8	−2·5	−3·0	−3·4	−3·6	−2·8	−0·6

NOTE.—When the sign is + the

determined from the selected quiet Days in 1892.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
Winter.												
′	′	′	′	′	′	′	′	′	′	′	′	′
43·3	44·1	43·4	42·1	41·1	40·2	39·6	39·1	38·5	38·1	38·1	37·9	38·2
43·5	44·1	44·2	43·4	41·3	40·2	40·8	40·0	39·1	38·2	37·9	37·7	37·8
43·8	45·1	44·2	42·9	41·0	39·7	38·6	38·0	38·0	37·2	37·0	37·9	38·0
39·2	41·0	41·0	39·7	37·8	36·5	35·8	34·6	34·3	33·6	33·5	33·5	33·5
36·3	37·1	37·0	36·3	36·1	35·2	34·7	34·3	33·9	33·1	33·3	33·1	33·1
37·0	37·7	36·5	36·2	35·1	34·8	34·9	34·1	33·3	32·7	32·2	32·0	32·2
40·5	41·5	41·1	40·1	38·7	37·8	37·8	36·7	36·2	35·5	35·3	35·4	35·5
Summer.												
′	′	′	′	′	′	′	′	′	′	′	′	′
40·8	42·1	42·3	40·7	39·1	38·2	37·6	37·4	37·4	37·1	37·1	36·8	36·5
44·0	45·1	43·8	42·0	40·1	38·1	36·9	36·5	36·8	37·3	37·5	37·2	36·9
42·7	44·2	44·0	42·1	40·4	38·9	37·8	36·9	36·8	36·6	36·8	36·5	36·2
41·3	43·3	43·7	42·4	39·9	38·0	36·7	36·4	36·4	36·8	36·7	36·5	36·5
43·5	44·8	43·6	41·6	38·7	36·6	35·8	36·2	35·9	35·9	35·9	35·5	34·9
43·8	43·8	42·5	40·5	38·4	37·0	36·7	36·5	35·9	35·6	36·1	35·9	35·5
42·7	43·9	43·3	41·6	39·4	37·8	36·9	36·7	36·5	36·5	36·7	36·4	36·1

Declination as derived from Table I.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
Summer Mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+5·9	+7·1	+6·5	+4.8	+2·6	+1·0	+0·1	−0·1	−0·3	−0·3	−0·1	−0·4	−0·7
Winter Mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+3·8	+4·8	+4·4	+3·4	+2·0	+1·1	+0·6	0·0	−0·5	−1·2	−1·4	−1·3	−1·2
Annual Mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+4·9	+6·0	+5·5	+4·1	+2·3	+1·1	+0·4	−0·1	−0·4	−0·8	−0·8	−0·9	−1·0

points to the west of its mean position.

Table III.—Hourly Means of the Horizontal Force in C.G.S.

Hours	Mid.	1.	2.	8.	4.	6.	6.	7.	8.	9.	10.
0·18000 +						Winter.					
1892. **Months.**											
Jan. ..	188	189	191	190	194	195	194	194	189	184	176
Feb. ..	195	190	195	189	187	189	189	195	194	186	179
March .	193	193	191	192	192	198	201	193	186	172	169
Oct. ..	209	208	210	212	212	213	214	212	204	192	183
Nov. ..	222	220	221	224	225	226	227	227	222	213	209
Dec. ..	210	209	210	211	216	216	218	218	215	212	206
Mean.	203	202	203	203	204	206	207	207	202	193	187
						Summer.					
April ..	201	201	200	200	199	199	198	194	186	175	166
May ..	219	216	213	214	212	214	208	200	190	183	181
June ..	224	221	221	221	221	219	213	203	194	190	191
July ..	201	200	198	198	197	194	189	183	179	174	167
Aug. ..	207	208	206	206	205	203	202	193	182	171	168
Sept. ..	196	196	194	193	193	192	189	179	169	164	162
Mean ..	208	208	205	205	204	204	200	192	183	176	172

Note.—During July, August, and September the Horizontal Force Magnetograph
quiet days (August 30) this disturbance was such as to make the indications of the instr
August depends on four days only.

Table IV.—Diurnal Range of the

Hours.	Mid.	1.	2.	8.	4.	6.	6.	7.	8.	9.	10.	
						Summer mean.						
	+ ·00007	+ ·00007	+ ·00004	+ ·00004	+ ·00003	+ ·00003	− ·00001	− ·00009	− ·00018	− ·00025	− ·00029	−
						Winter mean.						
	+ ·00001	·00000	+ ·00001	+ ·00001	+ ·00002	+ ·00004	+ ·00005	+ ·00005	·00000	− ·00009	− ·00015	−
						Annual mean.						
	+ ·00004	+ ·00004	+ ·00003	+ ·00003	+ ·00003	+ ·00004	+ ·00002	− ·00002	− ·00009	− 00017	− ·00022	−

Note.—When the sign i

(corrected for Temperature), as determined from the selected Days in 1892.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
Winter.												
179	184	188	188	189	190	195	197	197	196	192	195	195
174	179	180	185	190	191	199	196	200	200	198	199	201
166	173	179	189	186	185	192	200	198	193	194	199	199
184	196	202	204	204	209	211	214	216	215	216	218	218
209	212	218	222	222	225	227	229	227	228	229	227	229
204	206	210	211	214	217	217	218	219	216	215	218	214
186	192	196	200	201	203	207	209	210	208	207	209	209
Summer.												
173	182	190	194	200	201	203	206	204	204	205	204	204
192	200	206	212	218	221	223	224	220	222	222	220	219
209	213	220	225	221	225	228	231	231	230	230	227	226
170	180	193	200	207	209	212	214	214	213	210	208	207
183	192	199	206	213	217	220	221	222	223	219	222	217
183	193	197	196	192	193	197	202	201	201	198	202	200
185	193	201	206	208	211	214	216	215	215	214	214	212

times disturbed by the building operations which were then in progress. On one of the selected doubtful. The results obtained on that date have therefore been omitted, and the mean for

Horizontal Force as deduced from Table III.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
Summer mean.												
− ·00016	− ·00008	·00000	+ ·00005	+ ·00007	+ ·00010	+ ·00013	+ ·00015	+ ·00014	+ ·00014	+ ·00013	+ ·00013	+ ·C0011
Winter mean.												
− ·00016	− ·00010	− ·00006	− ·00002	− ·00001	+ ·00001	+ ·00005	+ ·00007	+ ·00008	+ ·00006	+ ·00005	+ ·00007	+ ·00007
Annual mean.												
− ·00016	− ·00009	− ·00003	+ ·0000l	+ ·00003	+ ·00006	+ ·00009	+ ·00011	+ ·00011	+ ·00010	+ ·00009	+ ·00010	+ ·00009

reading is above the mean.

Kew Observatory. Account of Receipts and Payments for the year ending December 31st, 1892.

Dr. RECEIPTS.	£ s. d.	£ s. d.
To Balance from Year 1891		380 6 10
Royal Society:—		
(Gassiot Trust).........	487 10 0	
(Loan for Building)	400 0 0	
		887 10 0
Meteorological Council:—		
Allowance.........	400 0 0	
Postages, &c.	3 3 11	
		403 3 11
Researches.........		2 4 6
Tests:—		
Verifications	1214 18 4	
Rating	633 18 8	
Lenses.........	7 17 9	
		1856 14 9
H.M.'s Office of Works, Share of Water Main.........		74 10 0
Commissions executed for Colonial and Foreign Institutions, &c.......		243 18 4
		£3848 8 4

Cr. PAYMENTS.	£ s. d.	£ s. d.
By Administration:—		
Superintendent	400 0 0	
Salaries, Wages, &c.	200 8 0	
Rent, Fuel, and Lighting.........	58 15 10	
Attendance, Cleaning, Repairs, Insurance, &c.......	184 12 10	
		843 11 8
Normal Observatory:—		
Salaries—Observations, Tabulations, &c.	296 12 0	
Incidental Expenses—Instruments, Postages, &c. ...	31 14 11	
		328 6 11
Researches:—		
Salaries—Observations, Reductions, &c.........	223 5 0	
Incidental Expenses—Instruments, Postages, &c. ...	2 11 0	
		225 16 0
Tests:—		
Salaries.........	858 17 7	
Incidental Expenses—Instruments, Porterage, Printing, &c. ...	183 15 2	
		1042 12 9
Extension of Premises:—		
West Wing	500 0 0	
Water Main from Richmond to the Observatory	156 10 0	
		656 10 0
Commissions executed for Colonial and Foreign Institutions, &c. ...		222 8 8
Balance:—		
Cash at Bank of England.........	327 18 10	
,, London and County Bank, Richmond.........	112 10 0	
,, Observatory (for Banking)............. 76 13 3		
,, ,, (Petty Cash) 12 0 8	88 13 11	
		529 2 9
		£3848 8 4

February 6, 1893.   Examined and compared with the vouchers, and found correct.   (Signed)   W. GRYLLS ADAMS, *Auditor.*

ESTIMATED ASSETS.

	£ s. d.	£ s. d.
By Balance as per Statement		529 2 9
Payments:—		
Meteorological Council—Allowance, Postage, &c.	109 14 11	
Test Fees	341 11 2	
Commission	34 16 8	
		486 12 9
Stock:—		
Blank Forms and Certificates	48 16 9	
Standard Thermometers	87 16 0	
		136 12 9
		£1152 8 8

ESTIMATED LIABILITIES.

	£ s. d.
To Administration accounts—Gas, Repairs, and Contingencies	40 1 2
Observatory accounts—A.G.B. Paper, Chemicals, &c.	35 17 3
Tests accounts—Fittings, Printing, Stationery, &c.	23 6 0
Unspent Balance of Pendulum Account	117 1 7
Royal Society (Loan)	400 0 0
Dorey & Co.—Balance for Building	40 0 0
Commissions	43 16 9
General Balance	452 5 6
	£1152 8 8

February 27, 1893.

(Signed)   T. W. BAKER,
Chief Assistant.

Table VII.—Hourly Means of the Inclination, calculat

Hours .	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
67° +						Winter.						
1892. Months.	′	′	′	′	′	′	′	′	′	′	′	′
Jan....	30·8	30·7	30·5	30·6	30·3	30·2	30·3	30·3	30·6	31·0	31·5	31·8
Feb....	29·7	29·9	29·5	30·0	30·1	30·0	30·1	29·7	29·7	30·2	30·6	30·8
March.	30·1	30·1	30·3	30·2	30·2	29·8	29·6	30·1	30·6	31·4	31·5	31·6
Oct....	28·6	28·6	28·5	28·4	28·4	28·4	28·4	28·6	29·1	29·9	30·3	30·3
Nov. ..	28·3	28·5	28·4	28·2	28·1	28·0	28·0	28·0	28·4	29·0	29·1	29·1
Dec....	28·6	28·6	28·6	28·5	28·2	28·3	28·1	28·1	28·3	28·5	28·9	29·0
Mean.	29·3	29·4	29·3	29·3	29·2	29·1	29·1	29·1	29·4	30·0	30·3	30·4
						Summer.						
April ..	29·5	29·5	29·5	29·5	29·6	29·7	29·8	30·1	30·6	31·1	31·6	31·4
May...	28·5	28·8	29·0	29·0	29·2	29·1	29·5	30·0	30·5	30·8	30·7	30·3
June ..	27·7	27·9	27·9	27·9	28·1	28·3	28·6	29·3	29·7	29·8	29·6	28·7
July...	29·2	29·2	29·3	29·4	29·5	29·8	30·0	30·3	30·5	30·8	31·1	30·9
Aug. ..	28·6	28·6	28·8	28·7	28·8	29·1	29·2	29·8	30·5	31·1	31·0	30·7
Sept. ..	29·7	29·7	29·9	30·0	30·0	30·1	30·3	31·0	31·6	31·7	31·7	31·1
Mean.	28·9	28·9	29·1	29·1	29·2	29·3	29·6	30·1	30·6	30·9	31·0	30·5

Table VIII.—Diurnal Range of the

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
						Summer Mean.						
	−0·4	−0·4	−0·2	−0·2	0·1	0·0	+0·3	+0·8	+1·3	+1·6	+1·7	+1·2
						Winter Mean.						
	−0·1	−0·0	−0·1	−0·1	−0·2	−0·3	−0·3	−0·3	0·0	+0·6	+0·9	+1·0
						Annual Mean.						
	−0·3	−0·2	−0·2	−0·2	−0·2	−0·1	0·0	+0·3	+0·7	+1·1	+1·3	+1·1

Note.—When the sign is +

rom the Horizontal and Vertical Forces (Tables III and V).

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
						Winter.						
′	′	′	′	′	′	′	′	′	′	′	′	′
31·3	31·0	30·8	30·9	30·8	30·7	30·4	30·2	30·1	30·2	30·4	30·2	30·2
30·9	30·7	30·7	30·5	30·2	30·1	29·6	29·8	29·5	29·4	29·5	29·4	29·3
31·4	31·0	30·8	30·3	30·6	30·6	30·1	29·5	29·6	29·9	29·8	29·4	29·4
30·1	29·3	29·1	29·1	29·2	28·9	28·7	28·5	28·3	28·4	28·3	28·1	28·1
29·0	28·8	28·5	28·3	28·4	28·2	28·0	27·9	28·0	28·0	27·9	28·0	27·8
29·0	29·0	28·8	28·8	28·6	28·4	28·4	28·4	28·3	28·5	28·5	28·3	28·6
30·3	29·9	29·8	29·6	29·6	29·5	29·2	29·0	28·9	29·1	29·1	28·9	28·9
						Summer.						
′	′	′	′	′	′	′	′	′	′	′	′	′
30·8	30·3	29·9	29·8	29·5	29·3	29·2	29·0	29·1	29·1	29·0	29·0	29·0
29·7	29·3	29·1	28·9	28·6	28·4	28·3	28·2	28·4	28·2	28·1	28·2	28·3
28·1	28·0	27·8	27·6	28·0	27·8	27·6	27·3	27·3	27·3	27·3	27·5	27·6
30·7	30·1	29·4	29·2	28·9	28·8	28·7	28·4	28·4	28·4	28·6	28·7	28·7
29·9	29·4	29·0	28·7	28·4	28·2	28·0	27·8	27·7	27·6	27·9	27·6	27·9
30·2	29·6	29·5	29·7	30·1	30·1	29·8	29;4	29·5	29·4	29·6	29·3	29·5
29·9	29·5	29·1	29·0	28·9	28·8	28·6	28·4	28·4	28·3	28·4	28·4	28·5

Inclination as deduced from Table VII.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
						Summer Mean.						
′	′	′	′	′	′	′	′	′	′	′	′	′
+0·6	+0·2	−0·2	−0·3	−0·4	−0·5	−0·7	−0·9	−0·9	−1·0	−0·9	−0·9	−0·8
						Winter Mean.						
′	′	′	′	′	′	′	′	′	′	′	′	′
+0·9	+0·5	+0·4	+0·2	+0·2	+0·1	−0·2	−0·4	−0·5	−0·3	−0·3	−0·5	−0·5
						Annual Mean.						
′	′	′	′	′	′	′	′	′	′	′	′	′
+0·8	+0·4	+0·1	−0·1	−0·1	−0·2	−0·4	−0·6	−0·7	−0·7	−0·6	−0·7	−0·6

the reading is above the mean.

APPENDIX II.—Table I.

Mean Monthly Results of Temperature and Pressure for Kew Observatory. 1892.

Months. 1892.	Thermometer Mean. °	Means of— Max. °	Means of— Min. °	Means of— Max. and Min. °	Absolute Extremes Max. °	Absolute Extremes Date. d. h.	Absolute Extremes Min. °	Absolute Extremes Date. d. h.	Barometer Mean. ins.	Barometer Absolute Extremes Max. ins.	Barometer Absolute Extremes Date. d. h.	Barometer Absolute Extremes Min. ins.	Barometer Absolute Extremes Date. d. h.	Mean vapour-tension. in.
Jan.	37·1	41·1	32·6	36·9	51·9	29 3 P.M.	23·8	16 2 A.M.	29·878	30·541	25 {11 P.M. / Midt.}	29·203	8 2 A.M.	·186
Feb.	39·3	43·9	34·4	39·2	52·9	7 3 „	19·2	17 7 „	29·810	30·545	13 9 A.M.	29·118	18 2 „	·200
March	37·4	43·5	31·8	37·7	59·2	18 2 „	22·2	9 6 „	30·029	30·596	31 8 „	29·261	15 6 P.M.	·172
April	48·3	56·5	36·9	46·7	68·7	5 3 „	27·9	15 7 „	30·017	30·526	1 1 „	29·503	16 5 A.M.	·211
May	54·5	63·9	45·1	54·5	80·7	31 2 „	31·2	7 4 „	30·005	30·409	12 6 „	29·632	3 6 P.M.	·291
June	57·7	66·4	48·9	57·7	80·7	10 1 „	38·2	15 4 „	30·006	30·429	8 9 „	29·525	23 9 A.M.	·388
July	59·5	67·4	52·3	59·9	77·0	3 Noon	46·7	21 5 „	30·018	30·353	24 8 „	29·548	20 0.25 „	·372
Aug.	61·3	69·4	53·4	61·4	79·3	17 5 P.M.	43·8	11 4 „	29·933	30·270	11 1 „	29·512	28 8 „	·410
Sept.	55·9	63·4	48·3	55·9	70·9	19 3 „	33·9	18 6 „	29·989	30·382	5 9 „	29·479	30 8 „	·359
Oct.	45·4	51·6	39·6	45·6	58·9	29 2 „	28·3	26 6 „	29·724	30·332	19 9 „	29·278	6 4 P.M.	·256
Nov.	45·2	49·7	39·6	44·7	60·1	14 1 „	30·0	2 8 „	30·057	30·496	27 10 P.M.	28·626	3 0.10 A.M.	·271
Dec.	36·6	40·9	32·2	36·6	54·0	15 Noon	17·9	27 8 „	30·002	30·381	16 9 A.M.	29·439	11 2 P.M.	·186
Yearly Means	48·0	54·8	41·3	48·1	..	.....	..	.....	29·956	..	.....	..	.....	·271

This Table is compiled from "Hourly Means," vol. 1892, of the Meteorological Office.

* Reduced to 32° at M.S.L.

Meteorological Observations.—Table II.

Kew Observatory.

Months.	Mean amount of cloud (0=clear, 10=overcast).	Rainfall.* Total. (in.)	Maximum. (in.)	Date.	Weather. Number of days on which were registered — Rain ‡	Snow.	Hail.	Thunderstorms.	Clear sky.	Overcast sky.	Gales.	Wind.† Number of days on which it was — N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm.
1892.																				
January	6·9	0·435	0·095	30	18	11	3	..	5	18	..	6	2	6	1	1	7	4	4	8
February	7·4	0·405	0·220	15	16	6	2	..	1	14	1	7	5	2	2	1	7	9	8	8
March	6·1	1·040	0·210	15	10	7	..	1	8	12	3	7	9	6	..	1	3	2	8	8
April	4·3	1·075	0·320	27	8	8	·1	..	14	4	3	7	9	2	..	1	8	6	1	8
May	5·7	1·470	0·300	25	12	..	1	3	9	8	..	8	4	3	1	6	8	4	2	4
June	6·0	2·790	1·170	28	13	..	..	3	6	10	..	8	3	3	..	5	10	5	1	..
July	7·0	2·075	0·470	5	9	..	..	1	4	15	..	6	7	2	2	3	5	4	3	1
August	6·5	3·280	1·795	27	14	..	..	4	4	13	..	2	1	3	1	4	10	5	5	8
September	7·0	3·180	1·005	29	14	..	..	2	2	15	..	2	..	1	..	3	15	5	4	8
October	6·6	3·680	1·090	30	22	..	..	..	3	11	..	8	2	1	1	8	8	6	2	6
November	7·8	2·710	0·980	15	19	..	..	..	2	20	..	6	..	4	3	7	5	4	1	10
December	6·8	1·180	0·330	1	18	..	..	..	5	15	..	4	..	9	2	..	5	8	3	7
Totals and means	} 6·6	24·270			163	27	6	14	61	165	4	68	42	42	13	34	88	65	31	52

* Measured at 10 A.M. daily by gauge 1·75 feet above ground.
‡ The number of rainy days are those on which 0·01 inch rain or melted snow were recorded.
† As registered by the anemograph.

Meteorological Observations.—Table III.

Kew Observatory.

Months.	Bright Sunshine. Total number of hours recorded.	Bright Sunshine. Mean percentage of possible sunshine.	Bright Sunshine. Greatest daily record.	Date.	Maximum temperature in sun's rays. (Black bulb *in vacuo*.) Mean.	Highest.	Date.	Minimum temperature on the ground. Mean.	Lowest.	Date. †	Horizontal movement of the air.* Average hourly velocity.	Greatest hourly velocity.	Date.
1892.	h. m.		h. m.		deg.	deg.		deg.	deg.		miles.	miles.	
January	34 24	13	5 42	25	60	85	24	26	16 {	9 / 16	9·9	31	29
February	48 42	17	6 12	18	‡	‡	‡	30	12	17	11·5	39	1
March	94 24	25	11 0	30	85	110	9	27	16 {	9 / 10	12·5	35 {	15 / 19
April	219 36	53	12 54	23	109	124	22	29	20 {	15 / 17	9·9	31	28
May	207 42	43	13 30	11	118	136	31	38	19	7	10·7	31	28
June	231 54	47	13 54	9	125	139	10	43	28	15	10·1	28	2
July	191 30	38	13 12	29	121	138	10	48	39	1	11·1	33	6
August	192 0	43	12 42	12	123	134	15	47	35	5	9·1	34	30
September	184 54	36	11 18	8	113	126	16	43	29	18	8·2	31	29
October	90 6	28	8 42	23	92	110	10	32	19 {	24 / 26	9·8	30	29
November	89 42	15	6 18	30	69	99	3	35	26	2	7·4	26	22
December	34 12	14	5 18	4	57	82	15	26	9	27	9·0	29	31
Totals and Means	1519 6	31	..	..	..	..	..	35	..	..	9·9	..	..

* As indicated by a Robinson's anemograph, 70 feet above the general surface of the ground.
† Read at 10 A.M., and entered to same day.   ‡ Instrument dismounted.

Table IV.

Summary of Sun-spot Observations made at the Kew Observatory.

Months.	Days of observation.	Number of new groups enumerated.	Days apparently without spots.
1892.			
January	10	9	—
February.............	14	9	—
March	16	13	—
April	19	16	-
May	17	14	—
June	17	17	—
July	16	13	—
August	16	15	—
September	17	15	—
October	17	13	—
November	10	11	—
December	9	13	—
Totals for 1892	178	158	—

APPENDIX III.—Table I.

RESULTS OF WATCH TRIALS. Performance of the 22 Watches which obtained the highest number of marks during the year.

Watch deposited by	Number of watch.	Balance spring, escapement, &c.	Mean daily rate.					Mean variation of daily rate. b.	Mean change of rate for 1° F.	Difference between extreme gaining and losing rates.	Marks awarded for			Total Marks. 0—100.
			Pendant up.	Pendant right.	Pendant left.	Dial up.	Dial down.				Daily variation of rate.	Change of rate with change of position.	Temperature compensation.	
			secs.	secs.	secs.	secs.	secs.	secs.	secs.	secs.				
Baume & Co., London	103018	Single overcoil, g.b., "tourbillon" chronometer	−0·8	−0·6	−0·7	−0·4	−0·2	0·26	0·03	2·3	34·8	39·3	17·8	91·9
Fridlander, Coventry	13400	Single overcoil, s.r., g.b lever	+0·1	+1·1	−0·7	+0·7	−1·1	0·4	0·06	4·0	32·8	37·0	16·2	86·0
Usher & Cole, London	27694	Single overcoil, s.r., fusee	+1·7	+2·1	+1·8	+4·2	+4·6	0·3	0·06	4·7	33·7	35·2	16·7	84·8
Rotherham & Sons, Coventry	98465	Single overcoil, s.r., g.b.	+2·0	+0·1	+0·2	+4·9	+1·9	0·5	0·03	7·5	30·0	33·9	18·0	83·9
Fridlander, Coventry	14564	Single overcoil, s.r, g.b centre seconds	+2·2	+8·1	+4·6	+4·0	+5·7	0·4	0·04	8·5	31·3	34·2	17·5	83·0
Jos. White & Son, Coventry	23579	Single overcoil, s.r., g.b.	−0·6	+0·8	+0·4	+1·1	+1·6	0·5	0·05	5·2	31·7	37·0	14·1	82·8
Fridlander, Coventry	02776	Single overcoil, d.r, g.b	+0·6	−0·8	+1·7	+1·1	+1·0	0·5	0·06	6·3	29·5	37·2	15·8	82·0
Jos. White & Son, Coventry	13132	Single overcoil, d.r., g.b. centre seconds	−0·6	−1·8	−0·4	−0·8	+2·6	0·6	0·03	5·0	27·7	36·9	17·7	82·1
Fridlander, Coventry	13521	Single overcoil, s.r, g.b.	+2·2	+5·6	+2·6	+4·2	+3·7	0·4	0·07	7·2	30·4	36·3	18·1	81·9
Fridlander, Coventry	52777	Single overcoil, d.r, g.b.	−0·6	+0·3	+0·5	+2·7	+2·0	0·4	0·08	6·6	31·4	35·6	14·7	81·7
Wellt & Co., London	6701	Single overcoil, d.r, g.b.	+3·6	+3·8	+3·0	+2·9	+8·8	0·8	0·03	5·0	24·7	38·8	18·1	81·6
Rotherham & Sons, Coventry	92771	Single overcoil, s.r, g.b	+1·5	+0·1	−2·7	+0·7	+0·9	0·5	0·05	0·7	29·5	35·4	16·6	81·5
Fridlander, Coventry	13394	Single overcoil, s.r, g.b.	−1·3	−4·3	−2·5	−0·0	−0·8	0·5	0·04	7·0	30·6	33·8	17·1	81·5
Holland, Rock Ferry	3563	Single overcoil, d.r, fusee, centre seconds	+2·1	+8·9	+2·7	+3·6	+3·6	0·6	0·04	5·7	28·6	35·7	17·1	81·4
Rotherham & Sons, Coventry	04468	Single overcoil, s.r, g.b	+1·6	+1·5	+0·5	+0·4	+3·0	0·6	0·06	9·0	27·6	36·0	17·2	80·8
Rotherham & Sons, Coventry	02777	Single overcoil, s.r, g.b.	+3·1	+2·3	+6·1	+1·0	+2·0	0·5	0·04	9·0	29·8	36·5	18·0	80·8
Klafsenberger, London	10950	Single overcoil, s.r, g.b. minute chronograph	−0·5	+0·0	+0·5	−1·3	+1·2	0·6	0·06	6·7	29·2	34·8	16·7	80·7
Fridlander, Coventry	52742	Flat sprung, s.r, fusee, traveller's (R.O.S. pattern)	−0·5	−2·8	+0·5	+0·5	−4·4	0·5	0·04	6·7	30·2	32·7	17·6	80·4
Usher & Cole, London	28037	Double overcoil, d.r., g.b. centre seconds	+2·6	+1·3	−2·6	+0·1	+0·9	0·6	0·02	7·5	27·5	34·4	18·5	80·4
Jos. White & Son, Coventry	33397	Single overcoil, s.r , g.b. centre seconds	−1·5	+3·1	+0·4	+1·5	+0·3	0·6	0·03	6·0	27·3	35·0	18·0	80·3
Usher & Cole, London	4444	Single overcoil, s.r., fusee, traveller's (R.O.S. pattern)	+4·7	+4·8	−0·0	+3·3	+3·7	0·5	0·06	7·7	29·7	24·6	16·0	80·3
Fridlander, Coventry	13666	Single overcoil, s.r , g.b., centre seconds	+2·1	+2·0	+5·9	+3·7	+1·3	0·6	0·05	5·8	27·7	34·9	17·7	80·2

In the above List, the following abbreviations are used, viz. :—s.r. for single roller; d.r. for double roller; g.b. for going barrel; + for gaining rate; — for losing rate.

APPENDIX III.—Table II.

Highest Marks obtained by Complicated Watches during the year.

Description of watch.	Number.	Received from.	Varia-tion.	Position.	Tempera-ture.	Total marks, 0—100.
Minute chronograph, minute repeater, and perpetual calendar with moon's phases	24987	S. Smith and Son, London	19·8	34·2	14·7	68·7
Minute and split seconds chronograph, and minute repeater	14492	H. Golay, London	22·1	33·2	6·7	62·0
Minute and seconds chronograph and minute repeater	52696	A. E. Fridlander, Coventry	30·6	33·7	14·1	78·4
,, ,, ,,	1979	A. Vuille, Chaux-de-Fonds	24·9	30·6	18·3	73·8
,, ,, ,,	4212	H. Golay, London	24·0	32·9	16·1	73·0
Minute and split seconds chronograph	3001	Baume and Co., London	28·4	32·6	16·8	77·8
,, ,, ,,	02013	S. Smith and Son, London	25·4	34·4	15·7	75·5
,, ,, ,,	13392	H. Golay, London	27·8	32·0	14·5	74·3
Minute and seconds chronograph	10950	Klaftenberger, London	28·2	36·8	15·7	80·7
,, ,, ,,	28106	Usher and Cole, London	26·5	35·2	16·4	78·1
,, ,, ,,	11437	Alf. M. Jacobs & Co., London	25·2	30·7	18·4	74·3
Ordinary seconds chronograph	97866	Rotherham and Sons, Coventry	25·9	33·4	14·1	73·4
,, ,, ,,	26768	Jos. Player, Coventry	22·7	32·0	18·1	72·8
,, ,, ,,	92489	Rotherham and Sons, Coventry	20·5	29·6	13·3	63·4
Minute repeater	82259	Rotherham and Sons, Coventry	24·2	29·5	16·7	70·4
,, ,, ,,	58782	A. E. Fridlander, Coventry	28·6	34·0	6·0	68·6
,, ,, ,,	17166	G. Oram and Son, London	19·6	35·8	12·5	67·9

April 20, 1893.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. " Magnetic Viscosity."   By J. HOPKINSON, D.Sc., F.R.S.,
E. WILSON, and F. LYDALL.   Received March 8, 1893.

The following experiments were carried out in the Siemens Laboratory, King's College, London, and are a continuation of experiments by J. Hopkinson and B. Hopkinson, a description of which appeared in the 'Electrician,' September 9, 1892.

In that paper determinations were given of curves showing the relation between the induction and the magnetising force, for rings of fine wire of soft iron and steel, through complete cycles with varying amplitudes of magnetising force, both with the ordinary ballistic method and with alternating currents of a frequency up to 125 complete periods per second.   It was shown that if the induction was moderate in amount, for example, 3000 or 4000, the two curves closely agreed; but, if the induction was considerable, for example, 16,000, the curves differed somewhat, particularly in that part of the curve preceding the maximum induction.   The difference was greater with steel than with soft iron.

It was not then determined whether this difference was a true time effect or was in some way due to the ballistic galvanometer.   The present paper is addressed to settling this point.

The ring to which the following experiments refer is of hard steel containing about 0·6 per cent. of carbon, in the form of wire $\frac{1}{100}$ in. diameter, varnished with shellac to ensure insulation.   The material was supplied by Messrs. Richard Johnson.   The ring is about 9 cm. diameter, and has a sectional area of 1·08 sq. cm.; it is wound with 200 turns of copper wire, and with 80 turns of fine wire for use with the ballistic galvanometer.

In the 'Electrician' paper the static curve of hysteresis was determined by the ballistic galvanometer, the connexions being made according to the diagram in fig. 1: where R is the hard steel wire *ring*, B is the ballistic galvanometer, S_1 is a reversing switch, and S_2 *is a small short*-circuiting switch for the purpose of suddenly insert-

Fig. 1.
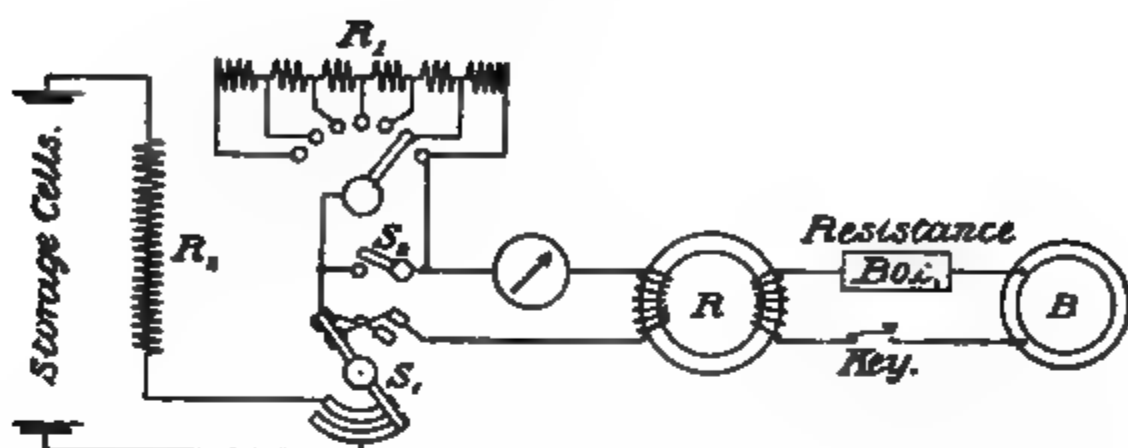
ing a resistance R_1 into the primary circuit. The resistance R_2 was so adjusted that the maximum current in the primary circuit was such as to give the desired maximum magnetising force on the ring.

In taking the kicks on the ballistic galvanometer the method adopted was as follows :—Having closed the primary by means of S_1, the switch S_2 was suddenly opened, thus allowing the magnetising force to drop to an amount determined by R_1, and the kick observed. A total reversal was then taken with S_1, and the kick again observed. The closing of S_2 again brought up the magnetising force to its maximum in the opposite direction to that at starting.

In a letter to the editor of the 'Electrician,' September 16, 1892, Mr. Evershed stated that "Had the slow cycle been obtained by the method described by Mr. Vignoles,* Messrs. Hopkinson would have found it in almost absolute agreement with the quick cycle curve."

To settle this point the static curve of hysteresis was obtained by the ballistic galvanometer, the connexions being made according to the diagram in fig. 2. This is not the method of experiment alluded

Fig. 2.
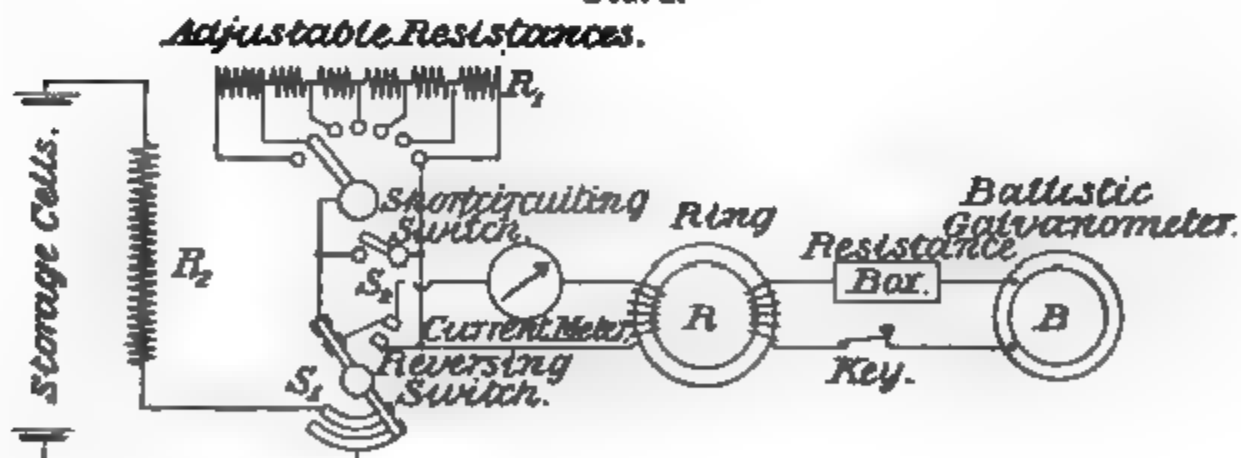
to by Mr. Evershed, but it is capable of varying the magnetising force in the same way as is described by him. B is the hard steel

* 'Electrician,' May 15 and 22, 1891.

the ballistic galvanometer, S_1 is a reversing switch,
. switch for the purpose of short-circuiting the adjust-
R_1.   The difference between this diagram and that in
l_1 can be suddenly inserted into the primary circuit by
the reversing switch S_1.   In this way it is possible to
etising force from one maximum through zero to any
vithin the other maximum by one motion of the switch
eration takes but a small fraction of a second to

5000
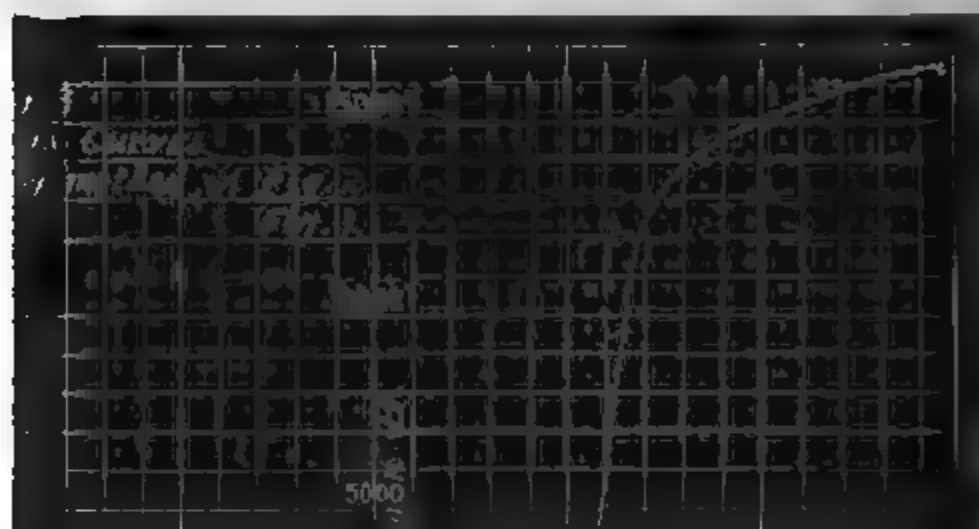
been plotted, and their close agreement proves that the differe
found between the static and quick cycle curves is not due to
cause suggested by Mr. Evershed. In each case the battery used
a potential difference of 108 volts, the periodic time of the balli
needle being 10 seconds.

It was observed, when taking the hysteresis curve by the meth
in fig. 2, that the sum of the inductions found by varying
magnetising force from one maximum to an intermediate point,
from that point to the other maximum, did not exactly equal
induction got by varying the magnetising force direct from
maximum to the other.

To investigate this with the ballistic galvanometer the magne
ing force (fig. 3) was taken from one maximum through zero to
point *a* by one motion of the reversing switch handle, and
galvanometer circuit closed at known intervals of time *after* s
change, the deflection being noted. This deflection does not rep
sent an impulsive electromotive force, nor yet a constant curre
but is caused by a current through the galvanometer diminishing
amount somewhat rapidly. It might arise from the comparativ
slow rate at which the magnetising current changes, owing to
self-induction of the circuit, or it might arise from a finite ti
required to develop the induction corresponding to a given magne
ing force. The former would be readily calculable if the ring ha
definite self-induction; in our case it is approximately calculable.

Let R be resistance of primary circuit, E the applied electromot
force, *x* the current, and I the total induction multiplied by
number of primary turns.

$$E = Rx + \frac{dI}{dt}.$$

Now I is known in terms of *x* for conditions of experiment v
approximately, and roughly dI/dt has a constant ratio to dx/dt
equal, say, to $L(dx/dt)$; hence the well known equation

$$E = Rx + L\frac{dx}{dt},$$

$$x = \frac{E}{R}\left(1 - \epsilon^{-\frac{R}{L}t}\right).$$

From our curves we see that induction per sq. cm. increa
10,000, whilst magnetising force increases 4. Total induction mu
plied by the primary turns, taking the volt as our unit, increa
$10,800 \times 200 \times 10^{-8}$, whilst the current increases $\frac{1}{2}$ an ampère, *i.e.*,

$$L = 4.32 \times 10^{-2}.$$

In the experiments made E = 4 and 108 volts and R = 0·8 and 21·6 ohms, whence

$$x = 5\left(1 - \epsilon^{-\frac{80}{4\cdot33}t}\right) \quad \text{and} \quad 5\left(1 - \epsilon^{-\frac{2160}{4\cdot33}t}\right).$$

In either case x does not differ sensibly from its final value when $t = \frac{1}{5}$ second. Hence the self-induction of the circuit can have nothing to do with the residual effects observed.

These experiments showed that an effect was produced upon the galvanometer needle, appreciable for some seconds, the effect being somewhat more marked with 4 than with 108 volts. But the whole amount was so small as to be less than 1 per cent. of the total change of induction; from which we infer that no material difference exists between curves of induction determined by the ballistic galvanometer and the inductions caused by magnetising forces operating for many seconds.

Effect of tapping the Specimen.—Having taken the magnetising force from its maximum through zero to the point a as before, the effect of tapping was marked, especially in the case of soft iron, when a kick corresponding to an acquirement of 633 lines of induction per sq. cm. was observed.

The following experiments on the hard steel wire ring were carried out with the alternator, the object being to ascertain if a time effect on magnetism exists. The ballistic curve (fig. 3) has been taken as a standard with which to compare the respective hysteresis curves. In each case the maximum magnetising force has been made as nearly as possible to agree with that used when taking the ballistic curve, and the method of test was that employed in the 'Electrician' paper. For the sake of completeness the diagram, fig. 4, and description are given over again.

Quoting from that paper, we have: "For determining the points on the closed curve of magnetisation given by rapid reversals of the current in the coil, the ring was connected in series with a non-inductive resistance to the poles of an alternate current generator, or a transformer excited by the generator, thus :—

FIG. 4.
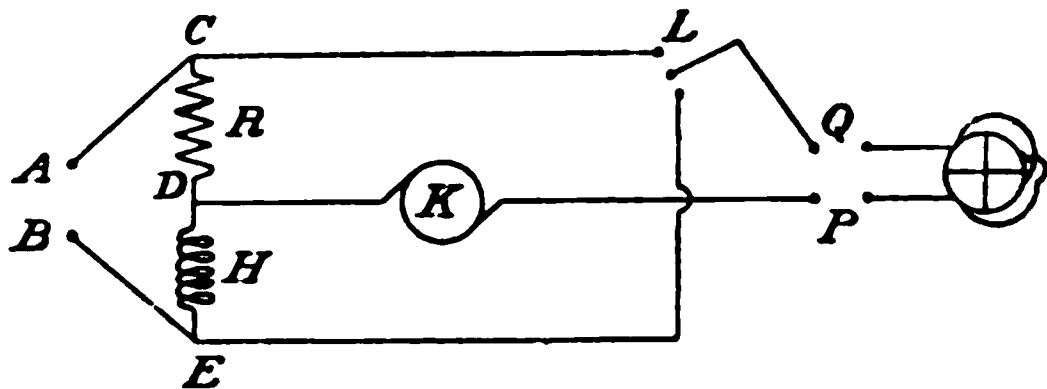
in which A, B are the poles of the transformer or generator; C, D the terminals of the non-inductive resistance R; H the coil surrounding the ring; P and Q the studs of a reversing key connected to the quadrant of a Thomson quadrant electrometer; L a key by means of which Q could be connected with C or E at will; and K a revolving contact maker, through which P was connected to D. A condenser was connected to P and Q, in order to steady the electrometer readings. The contact maker K was bolted on to the axle of the generator. It consists of a circular disc of ebonite, about 13 in. in diameter, having a small slip of copper, about $\frac{1}{16}$ in. wide, let into its circumference. A small steel brush presses on the circumference, and makes contact with the piece of copper once in every revolution. The position of the brush can be read off on a graduated circle. The quadrant electrometer thus gives the instantaneous value of the difference of potential between the points C and D, or the points D and E, according to the direction of the key L."

Frequencies of 5, 72, and 125 ⁓ per second have been tried, two values being given to the potential difference at the terminals of the alternator in each of the frequencies 72 and 125, making in all 5 complete experiments. The curves so obtained are given in figs. 8, 9, 10, 11, and 12 respectively. From observations of the values of the electromotive force between C and D (fig. 4) at different times in the period, a curve A (in each experiment) was plotted, giving the magnetising force in terms of the time; a similar curve was plotted for the electromotive force between D and E, which, when corrected by subtracting the electromotive force due to the resistance of the coil H, gives the potential or time rate of variation of the induction in terms of the time. Hence the area of this curve (B) up to any point, *plus* a constant, is proportional to the induction corresponding to that point. This is shown in curve C, which is the integral of B. In each of the five experiments the ring with the non-inductive resistance was placed across the terminals of the alternator, and the excess of potential taken up by a non-inductive resistance.

In fig. 5 the hysteresis curves for frequencies of 5, 72, and 125 are compared with the ballistic curve. These curves are marked 5, 72L, and 125L respectively. The corresponding values for B and H, from which these curves have been plotted, are given in Tables II, III, V, which have been obtained from the curves in figs. 8, 9, and 11 respectively.

The most noteworthy features in these curves are that the curve with a frequency of 5 is very near the ballistic curve, if allowance is made for difference in the magnetising current, and that the curves with a frequency of 72 and 125 deviate very materially, particularly in the part of the curve somewhat preceding the maximum induction. Hence the time effect mainly develops with a greater frequency than

5 per second.   Hence also we infer that this effect, as already described in the 'Electrician,' is a true time effect, not arising in any way from the ballistic galvanometer.

In fig. 6 the hysteresis curves for a frequency of 72 are compared with the ballistic curve. The curves are marked 72L and 72H respectively, the potentials at the terminals of the alternator in the two cases being approximately 36 and 430 volts.   The corresponding values for B and H are given in Tables III, IV, which have been obtained from the curves in figs. 9 and 10 respectively.

The difference between the two curves in fig. 6 was at first puzzling, but a little consideration satisfied us that it arises from the same time effect. The curve 72L was determined three times, with the same result. The numerals refer to thirtieths of a half-period. From 26 to 28·8 of the L curve the magnetising force increases from 31·8 to 45·6, whilst from 21 to 26 of the H curve it increases from 30·6 to 44, the rate of change being about double as great in the former case as in the latter, and it is the L curve which deviates most from the ballistic curve. In like manner, in the neighbourhood of zero induction, the induction in the H curve is changing twice as fast as the induction of the L curve, and it is here the H curve which differs most.   How these differences of rate of change arise can be seen by inspecting figs. 9 and 10.

In fig. 7 the hysteresis curves for a frequency of 125 are compared with the ballistic curve. The curves are marked 125L and 125H respectively, the potentials at the terminals of the alternator being approximately 62 and 750 volts. The corresponding values for B and H are given in Tables V and VI, which have been obtained from the curves in figs. 11 and 12 respectively.

These curves show the same difference as fig. 6, but less markedly than in fig. 5.   The L curve was determined twice.

Experiments have been made upon chromium steel, supplied by Mr. Hadfield, having the following composition:—0·71 per cent. carbon, 9·18 per cent. chromium, when annealed, and when hardened by raising to low yellow and plunging into cold water.   The results show that the same time effect exists in this case, although it was not so marked as in the case of the hard steel.

We draw the following conclusions from these experiments :— (1.) As Professor Ewing has already observed, after sudden change of magnetising force, the induction does not at once attain to its full value, but there is a slight increase going on for some seconds.   (2.) The small difference between the ballistic curve of magnetisation with complete cycles and the curve determined with a considerable fre- *quency,* which has already been observed, is a true time effect, the *difference being* greater between a frequency of 72 per second and *5 per second* than between 5 per second and the ballistic curve.

Fig. 5.
Hysteresis Curves.

Fig. 6.
Frequency 72 ~
1 mm vertical = 35 Amperes
D ... = 1.01 Volts
C ... = 480 Units of Induction
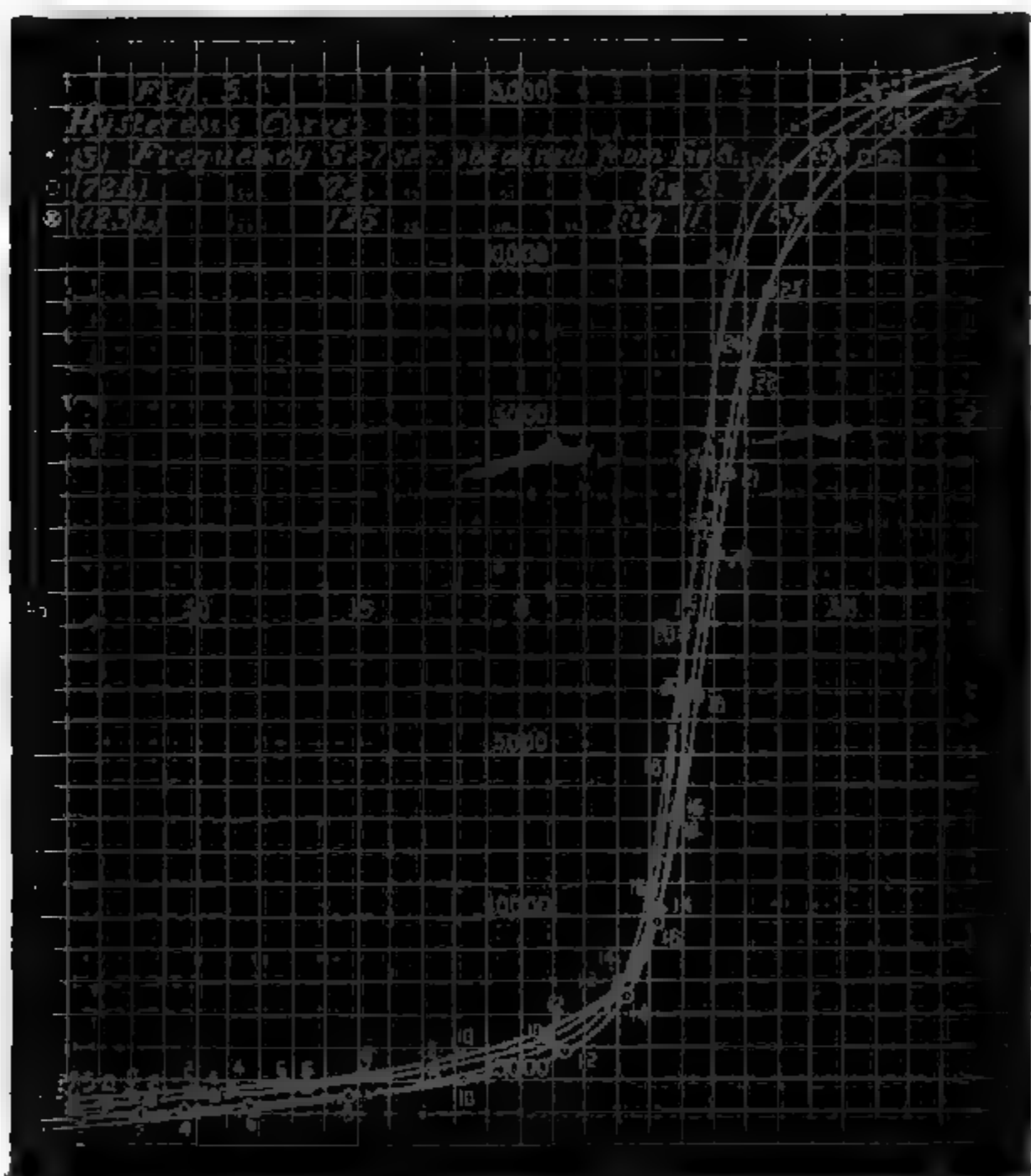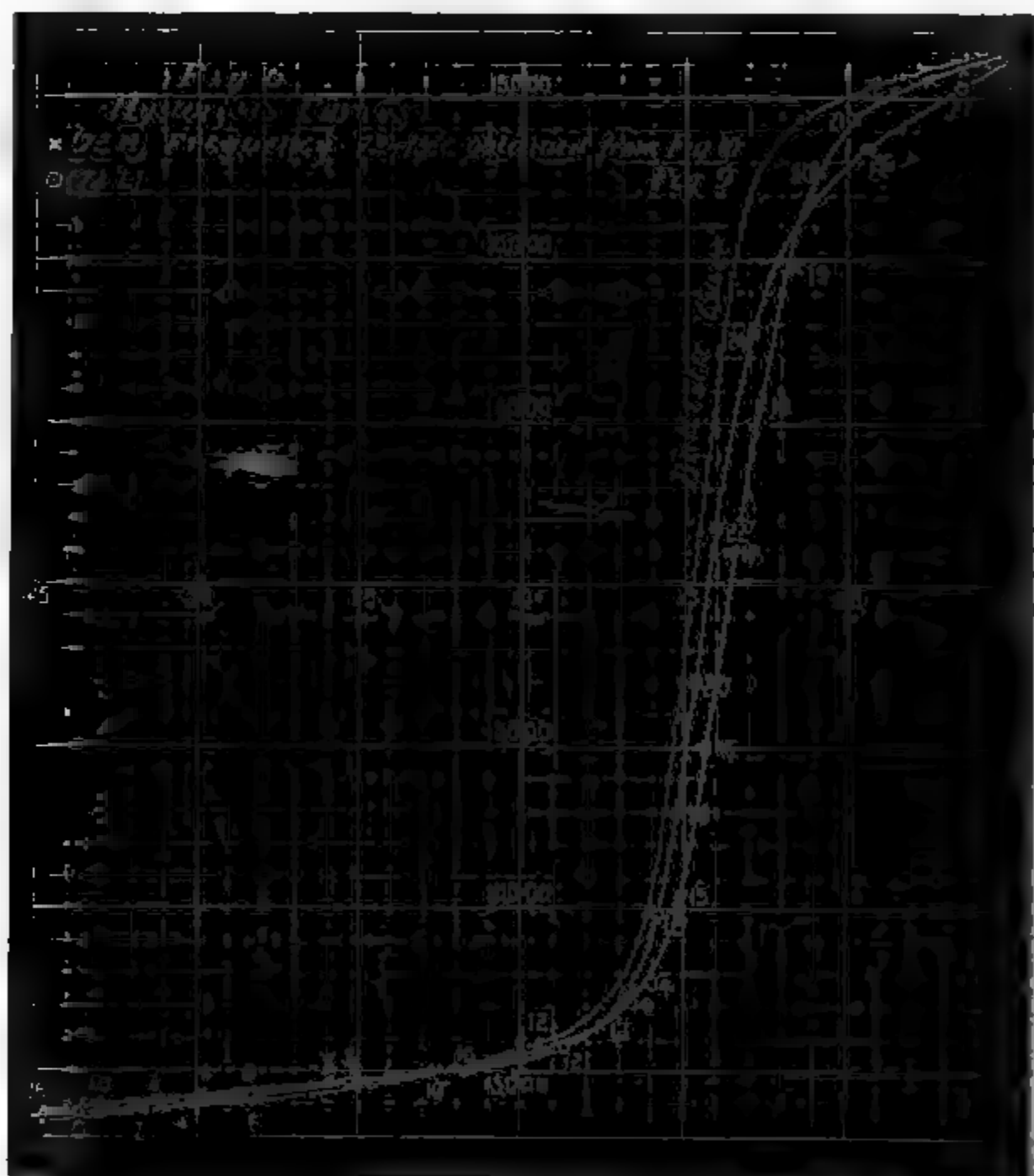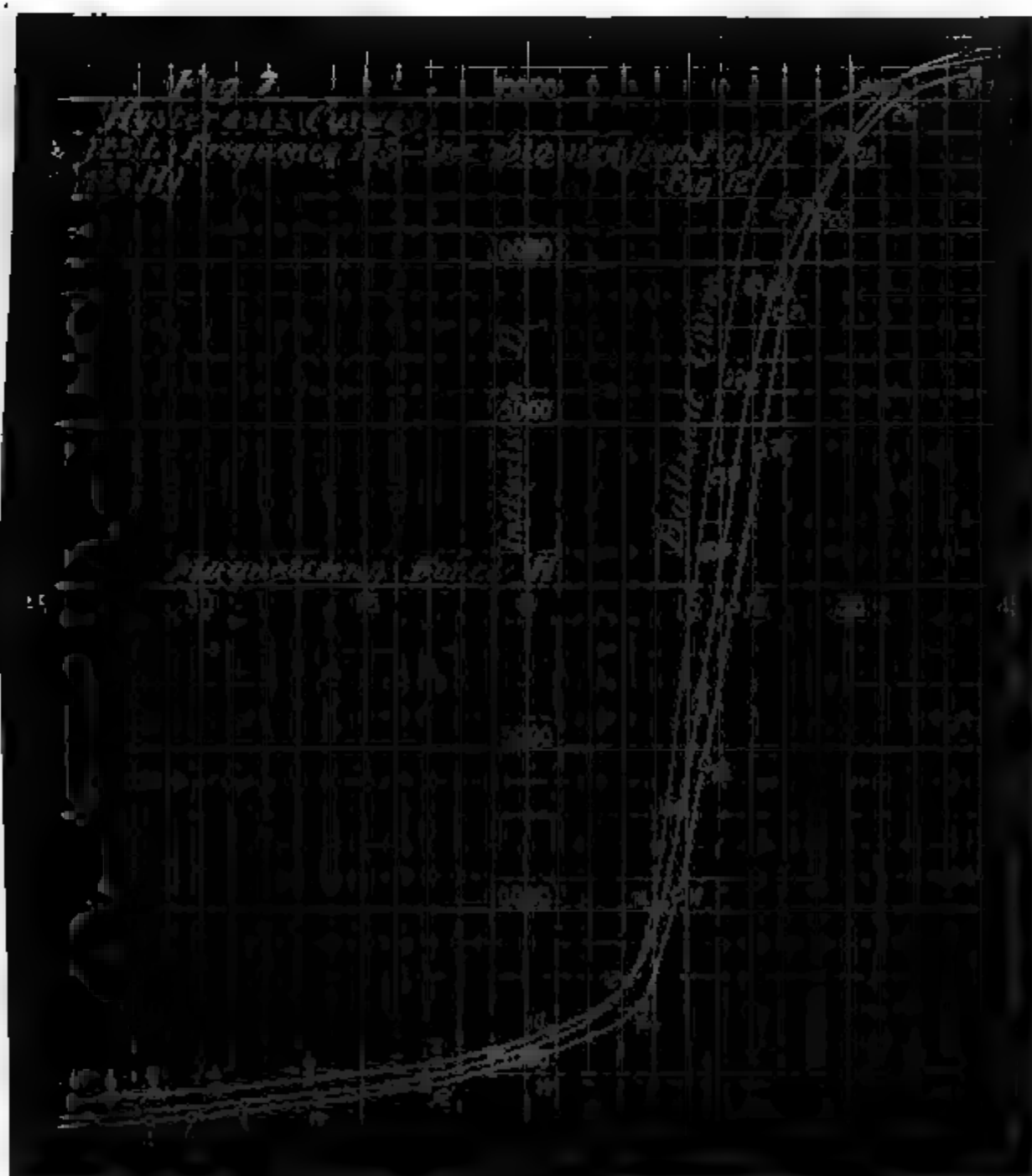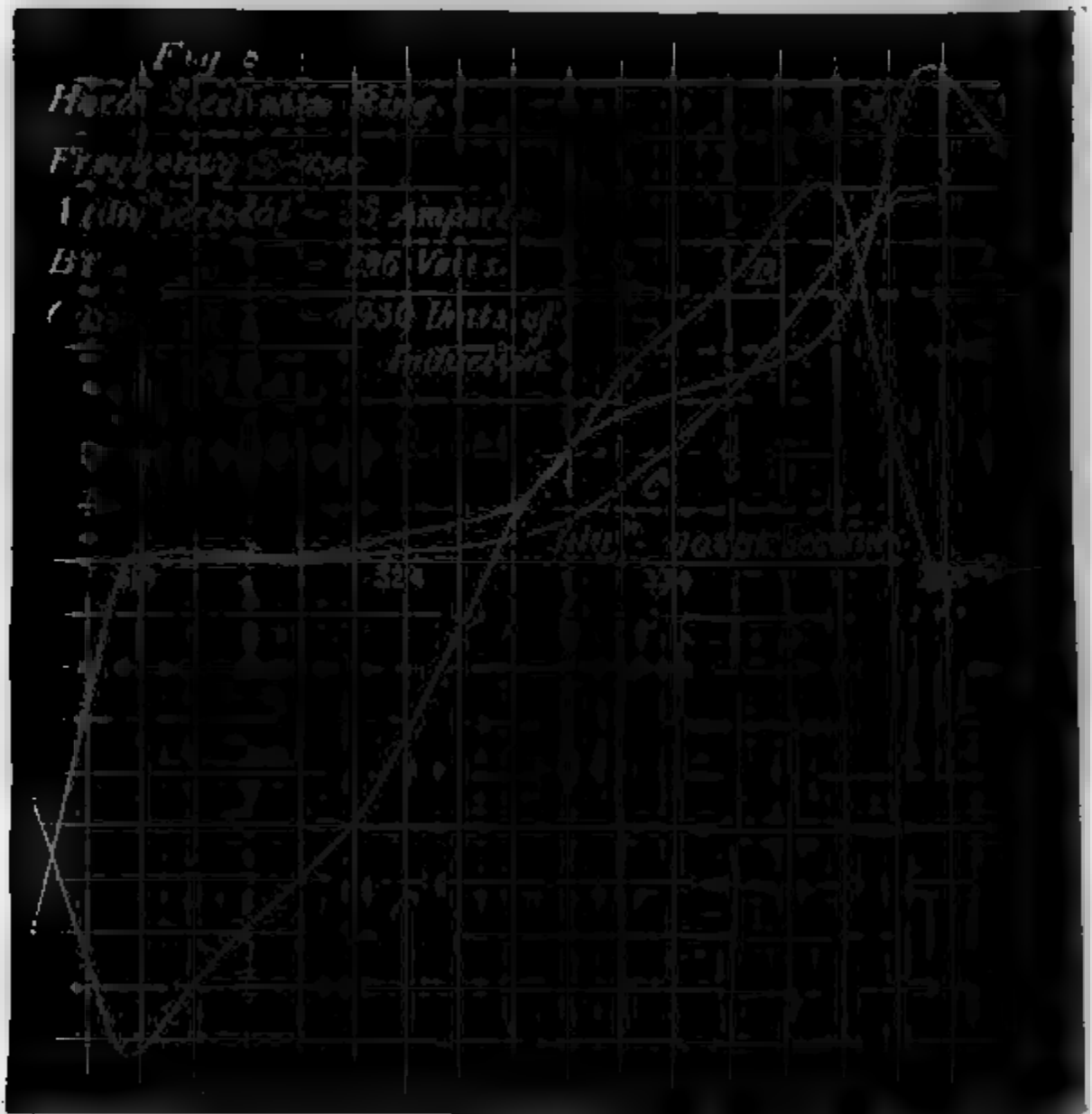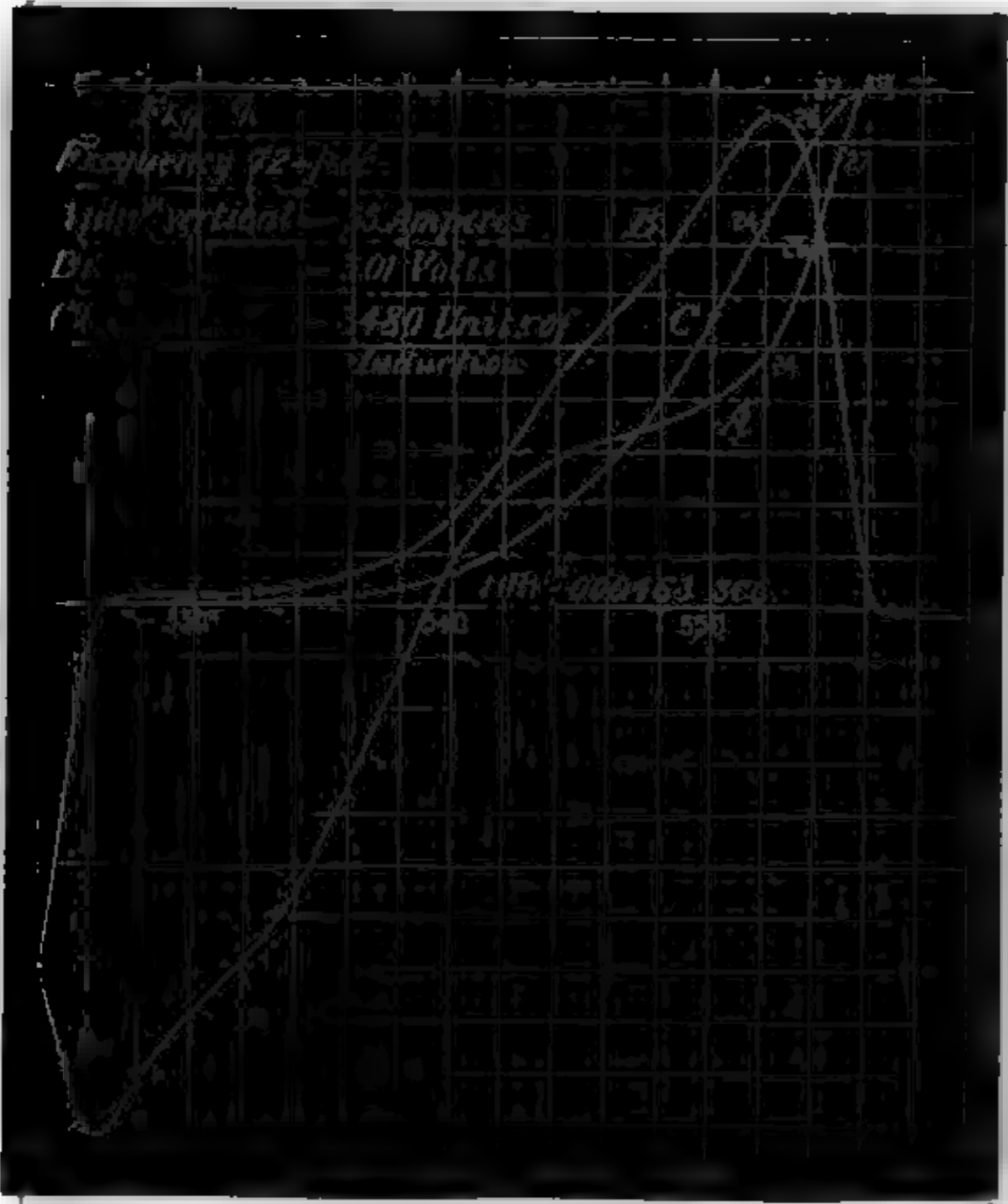
inson and Messrs. Wilson and Lydall.   [Apr. 20,

Fig. 11.
Frequency 185 ~sec
Exciting current = 550 Amperes
508 Volts
9390 Units of Induction
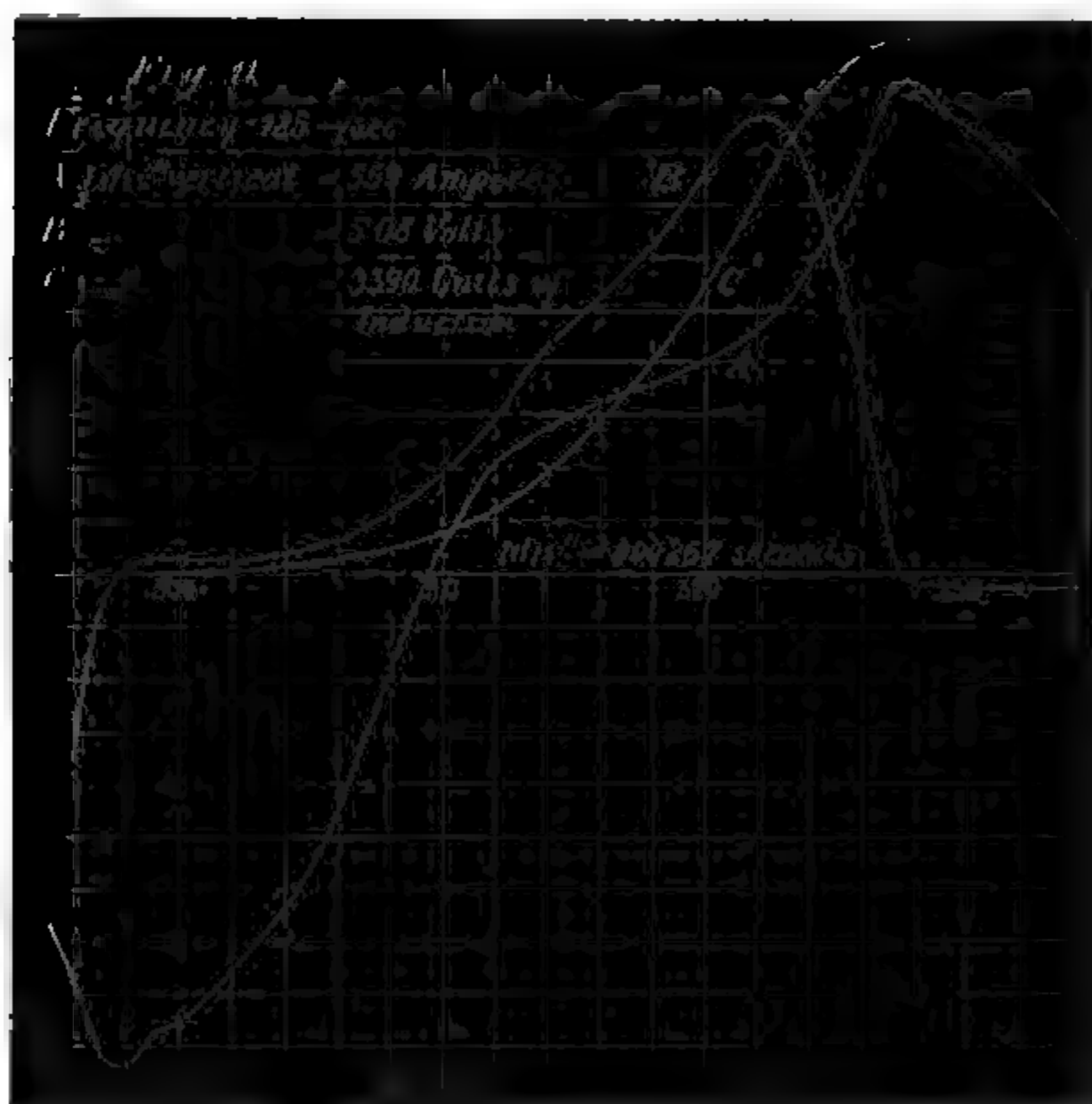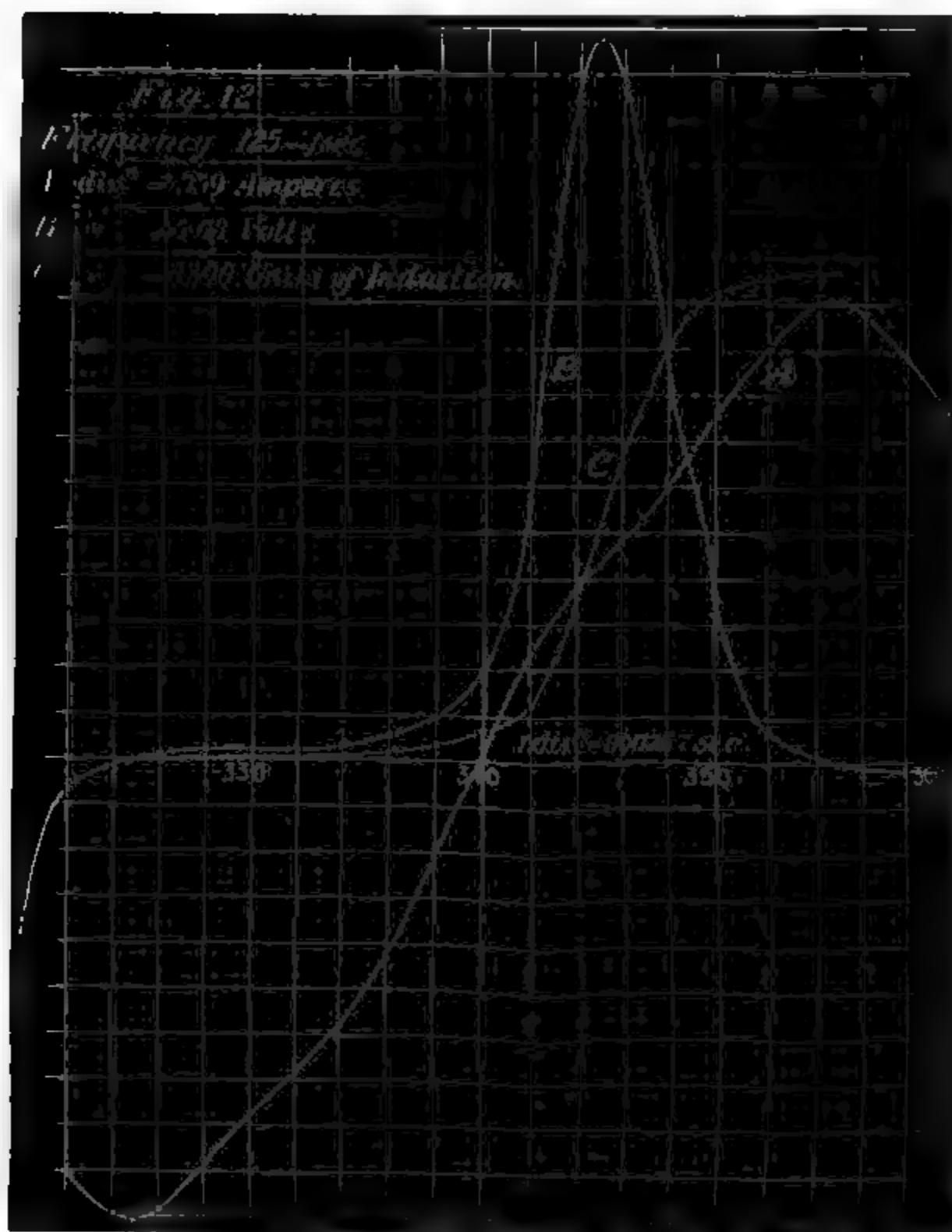
Table I.—Hard Steel-wire Ring.

H.	B.	
	Points marked × obtained by method shown in fig. 1.	Points marked • obtained by method shown in fig. 2.
+ 44·12	16,295	16,436
34·77	15,830	15,650
25·44	+ 14,407	14,290
19·32	..	9,539
16·1	..	+ 1,704
14·2	− 4,045	− 4,130
7·73	12,690	12,820
0	..	14,280
− 7·73	14,870	14,990
14·2	15,270	15,380
16·1	..	15,460
19·32	..	15,630
25·44	15,733	15,860
34·77	16,033	16,150
44·12	16,295	16,436

Table II.—Frequency 5 per second.

B.	H.
15,660	41·7
15,200	34·4
13,010	25·1
10,190	19·95
+ 3,970	17·1
− 1,230	15·3
5,340	14·1
9,176	12·3
12,007	9·4
13,377	+ 3·4
14,382	− 5·6
14,747	14·4
15,203	22·0
15,295	26·85
15,477	30·86
15,523	35·75
15,660	41·7

Table III.—Frequency, 72 per second. Potential at Terminals of Alternator, approximately 36 Volts.

B	H.
16,245	+ 45·7
16,180	45·5
15,215	39·9
13,410	31·7
7,805	21·3
+ 1,805	17·8
− 3,030	15·6
7,027	14·3
10,121	12·6
12,506	9·86
14,118	4·26
14,956	− 5·44
15,407	15·86
15,729	24·77
15,923	31·1
16,118	35·4
16,219	40·9
16,245	45·7

Table IV.—Frequency, 72 per second. Potential at Terminals of Alternator, approximately 430 Volts.

B.	H.
16,221	43·98
16,214	44·32
16,069	42·75
15,919	40·61
15,685	37·25
15,299	34·33
14,299	30·97
12,689	27·6
9,839	24·91
5,589	21·88
+ 999	19·3
− 5,073	16·6
9,609	14·36
12,300	11·22
13,530	7·18
14,145	+ 2·24
14,991	− 8·06
15,452	17·72
15,644	25·13
15,814	29·62
15,914	33·66
16,122	38·37
16,221	43·98

Table V.—Frequency, 125 per second. Potential at Terminals of Alternator, approximately 62 Volts.

B.	H.
15,936	+ 41·74
15,746	40·95
15,119	35·00
13,739	30·07
11,732	26·48
9,222	23·11
6,462	20·87
3,576	19·19
+ 1,192	18·18
− 3,136	16·16
6,776	14·59
9,850	12·57
12,172	9·20
13,615	2·24
14,618	− 6·06
15,120	19·75
15,434	28·61
15,622	34·10
15,773	36·37
15,936	41·74

Table VI.—Frequency, 125 per second. Potential at Terminals of Alternator, approximately 750 Volts.

B.	H.
16,689	+ 45·1
16,671	44·65
16,565	40·72
15,311	33·66
13,930	30·18
11,544	27·15
8,411	23·78
+ 4,077	21·54
− 565	19·30
5,396	16·83
9,474	14·36
12,862	10·77
14,368	1·12
15,309	− 9·20
15,873	18·85
16,099	26·25
16,262	30·63
16,413	34·33
16,564	39·49
16,670	43·98
16,689	45·10

II. " On the Spectrum of Thallium, and its Relation to th
Homologous Spectra of Indium and Gallium." By HENR
WILDE, F.R.S. Received March 14, 1893.

The spectral reactions of thallium have been made the subject
observation by so many distinguished physicists as to leave little
be gleaned by further research of the spectrum of this interestin
element. A strong line in the red seems, however, to have bee
overlooked, with one exception, in all the tables of the spectrum
thallium which have hitherto been published. Huggins would appe
to have observed this line in the spark spectrum, and assigned a pla
for it in his table* corresponding to a wave-length of 6547 of Ån,
ström's scale.†

For reasons which will presently appear, the existence of this li
has not been confirmed by Thalén in the spark spectrum, nor l
Liveing and Dewar in the arc spectrum,‡ nor by other subseque
observers.

The importance of this line in relation to the homologous spect
of other elements of the same series induced me to undertake e
periments which have proved, beyond doubt, the existence of th
characteristic spectral reaction of thallium.

When metallic thallium or its chloride is volatilised in the ele
tric arc between carbon points, a red line appears in the spectru
apparently coincident with the hydrogen line C 6562. As the
line invariably appears in the spark spectrum of all metallic su
stances in moist air, the red thallium line was considered by m
as it would doubtless have been by others, as due to the electr
lysation of aqueous vapour suspended in the atmosphere. It wa
however, found that the line did not present itself in the a
spectrum of the alkaline and other metals experimented with und
the same conditions. It therefore appeared to me probable th
the red line belonged to the spectrum of thallium. There wa
moreover, the further fact that, up to the present time, the a
spectrum of thallium below the ultra-violet was limited to the we
known Crookes' green line, and in this respect was anomalous in t
simplicity of its spectrum at the high temperature of the electric a

That the red line was not due to electrolytic hydrogen of aqueo
vapour was shown by the following experiments:—Two wid
necked phials were partially filled with strong sulphuric aci
leaving an air space at their upper ends of about two cubic inche

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* ' Phil. Trans.,' vol. 154, p. 152, 1864.
† Watts' ' Index of Spectra,' 1889.
‡ ' Brit. Association Report,' 1885.

The mouths of the phials were closed with tightly-fitting stoppers of caoutchouc, through which pairs of copper wires were thrust for the purpose of attaching electrodes of different metals in the interior of the phials. Pairs of platinum and thallium electrodes were placed in the phials respectively, which were set aside for some hours to allow the moisture of the enclosed air to be absorbed.

An induction coil, giving a 10-inch spark in air with 12 ampères of current, was used in the experiments, and the density of the spark was increased by connecting the secondary circuit with the coatings of a Leyden jar in the usual manner.

The observations were made with a direct-vision spectroscope of five prisms, fitted with an illuminated scale, which enabled comparisons to be made simultaneously between the spectral lines of different substances with great exactness and rapidity. The range of the instrument with the arc spectrum included the rubidium line 7951, and the calcium line H^2 3933, while the prismatic dispersion was such as to well divide the potassium double line 4044, 4042.

On transmitting a succession of sparks from the platinum electrodes through the dried air in one of the phials, the air lines 6602 and 6482 were very conspicuous, but there was no trace of the hydrogen line 6562 between them. When, however, the sparks were taken from the thallium electrodes under like conditions the sharp red line appeared as in the arc spectrum. The same results were obtained when similar pairs of platinum and thallium electrodes were connected in series and placed under a large glass receiver, the air in which had been dried by sulphuric acid.

The red line of thallium does not appear in the oxy-hydrogen flame, nor when the intensity of the spark is somewhat reduced by placing a vacuum tube in series with the thallium electrodes, although the brilliancy of the air lines was but little diminished.

That the red line was not due to hydrogen occluded in the thallium was shown by the following experiments:—A pair of palladium electrodes were saturated with electrolytic hydrogen from dilute sulphuric acid, through which an electric current was transmitted for several minutes. These electrodes were substituted for those of platinum in the phial of dried air; and when the sparks passed between them the strong hydrogen line C appeared for several seconds before it finally disappeared through the heating of the palladium electrodes.

A small quantity of thallium was placed on a piece of pumice under the glass receiver of an air pump, and fused *in vacuo* by the arc light condensed by a pair of Fresnel annular lenses focussed through the side of the receiver. The spectrum of the thallium fused *in vacuo*, from which all gas had been excluded, was the same as in the previous experiments.

In all my observations on the spectrum of thallium I found th
the red line was slightly more refrangible than the hydrogen lin
The difference, however, was so small from the nebulous character
hydrogen C at atmospheric pressure, that, with the spark spectru
in moist air, the thallium line overlapped C so much as to be undi
tinguishable; but when the intensity of the hydrogen line was r
duced, by passing the spark through partially dried air, the two lin
could be distinctly recognised by the difference in their brigh
ness.

A closer comparison of the red lines of thallium and hydrogen w
now made by placing the narrow part of a hydrogen vacuum tul
behind the phial containing the thallium electrodes.

The sparks through the vacuum tube and electrodes were tak
simultaneously from separate induction coils. The red lines we
now distinctly separated by a dark space of less width than the i
terval between the double sodium line D^1D^2. The spectrum of C w
less bright than the thallium line, owing to the greater distance of t
vacuum tube from the slit of the spectroscope; but a more distin
line was obtained by substituting for the hydrogen tube one of car
onic acid, in which the C line appeared by the electrolysation of t
residual aqueous vapour contained in the gas.

The wave-length of the thallium red line, as estimated by the d
ference of refrangibility of the hydrogen line, from the same electrod
in moist air, is 6560; or, 6558, as estimated from the distinct line
hydrogen in a vacuum tube.

A comparison of the arc spectrum of thallium may now be ma
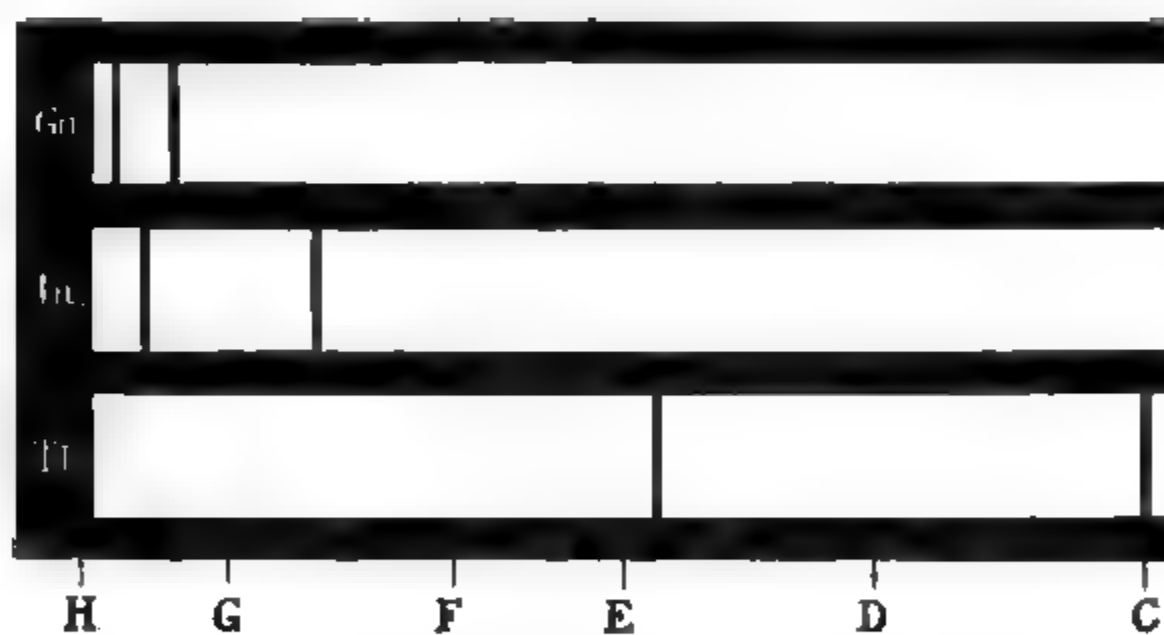
with the similar arc spectra of its analogues, indium and gallium.
have already pointed out that the characteristic lines of the alkal
metals and their homologues of position in the thallium v

2 D 2

advance towards the more refrangible parts of the spectrum in the inverse order of their atomic weights.[*] The correlation of the spectral reactions of thallium, indium, and gallium with the other properties of these elements is of further interest from the fact that their arc spectra (below the ultra-violet) are represented by homologous pairs of lines in the order of their atomic weights. $Tl = 204$; $\lambda 6560, 5349$. $In = 113\cdot4$; $\lambda 4510, 4101$. $Ga = 70$; $\lambda 4170, 4031$. The intervals of space between each homologous pair of lines, as will be seen, increase in the same order. These relations are further represented in the subjoined diagram, reduced from the scale of Ångström's normal spectrum.

It would be interesting to know if the arc spectrum of scandium is represented by a similar pair of lines in the ultra-violet, as I have already suggested in the paper referred to before this elementary substance was discovered.

III. " The Potential of an Anchor Ring." By F. W. DYSON, M.A., Fellow of Trinity College, Cambridge, Isaac Newton Student in the University of Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received March 16, 1893.

(Abstract.)

1. This paper is a continuation of some researches on rings published in the ' Phil. Trans.,' 1893. In that paper the potential of an anchor ring was found at all external points; here it is determined for internal points. The annular form of rotating gravitating fluid was considered; the stability of such a ring is investigated here. In addition, the potential of a ring of elliptic cross-section, being of interest in connexion with Saturn, is obtained. Besides this, the similarity of the methods and of the analysis employed has led me to put in this paper also the determination of the steady motion of a single vortex ring of finite cross-section and the motion of several fine vortex rings on the same axis.

2. Let the figure represent a section through the axis Oz of an anchor ring. O is the centre of the ring, C the centre of the cross-section, P any point inside the ring.

Let $OC = c$, $CA = a$, $CP = R$, $\angle ACP = \chi$.

Then it is shown that
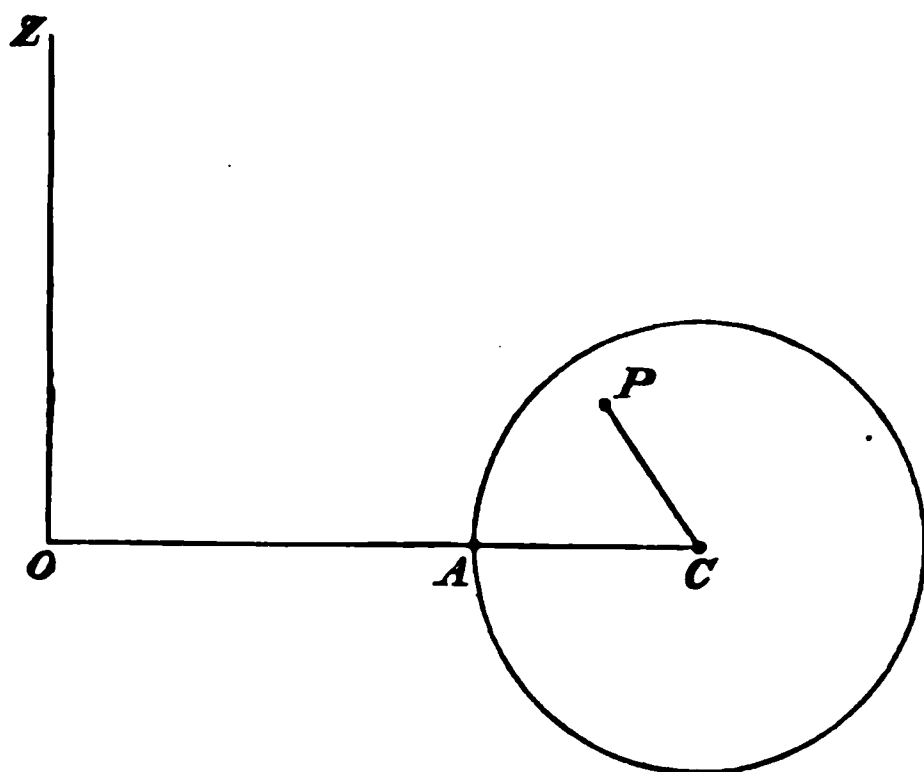
$$\frac{R^n}{a^n}\cos n\chi+\frac{a}{2c}\cdot\frac{R^{n+1}}{a^{n+1}}\cos \overline{n-1}\chi+\left(\frac{a}{2c}\right)^2\cdot\frac{R^{n+2}}{a^{n+2}}\left\{\tfrac{1}{2}\frac{2n+1}{2n+2}\cos n\chi+\frac{1.3}{2.4}\right.$$

$$\left.\cos \overline{n-2}\chi\right\}+\cdots +\left(\frac{a}{2c}\right)^p\cdot\left(\frac{R}{a}\right)^{n+p}\left\{\tfrac{1}{2}\frac{(2n+1)\cdots(2n+2p-3)}{(2n+2)\cdots(2n+2p-2)}\right.$$

$$\cos (n+p-2)\chi+\frac{p-1}{1}\cdot\frac{1.3}{2.4}\cdot\frac{(2n+1)\cdots(2n+2p-5)}{(2n+2)\cdots(2n+2p-4)}\cos (n+p-4)\chi$$

$$+\frac{(p-1)(p-2)}{2!}\cdot\frac{1.3.5}{2.4.6}\cdot\frac{(2n+1)\cdots(2n+2p-7)}{(2n+2)\cdots(2n+2p-6)}\cos (n+p-6)\chi$$

$$\left.+\cdots\right\}+\&c.$$

is a solution of Laplace's equation.

A similar expression holds when for cosines we write sines.

Writing now

$$\sigma=\frac{a}{c}\quad\text{and}\quad L=\log\frac{8c}{a},$$

the potential of a solid anchor ring at internal points is found as far as terms in σ^4. As far as terms in σ^2 it is given by

$$V=2\pi a^2\left\{L+\tfrac{1}{2}\left(1-\frac{R^2}{a^2}\right)+\sigma\left[\frac{L-1}{2}\cdot\frac{R}{a}-\frac{R^3}{8a^3}\right]\cos \chi+\sigma^2\left[-\frac{L-\frac{1}{4}}{16}\right.\right.$$

$$\left.\left.+\frac{L-1}{8}\cdot\frac{R^2}{a^2}-\tfrac{3}{64}\frac{R^4}{a^4}+\left(\frac{3(L-\frac{5}{4})}{16}\frac{R^2}{a^2}-\tfrac{5}{96}\cdot\frac{R^4}{a^4}\right)\cos 2\chi\right]\right\}.$$

The work done in collecting the ring from infinity is

$$\frac{M^2}{2\pi c}\left\{L+\tfrac{1}{4}-\frac{L-\frac{2}{3}}{8}\sigma^2-\frac{3(L-\frac{19}{4})}{512}\sigma^4-\cdots\right\}.$$

A distribution of matter on the ring of surface density $\beta_n \cos n\chi$ gives on the ring the potential V where

$$\frac{V}{2\pi a\beta_n} = \frac{\cos n\chi}{n} + \frac{\sigma}{4n} \left\{ \frac{\cos (n+1)\chi}{n+1} - \frac{\cos (n-1)\chi}{n-1} \right\}$$

$$+ \frac{\sigma^2}{16n} \left\{ \frac{2n+3}{(n+1)(n+2)} \cos (n+2)\chi - \frac{2n-3}{(n-1)(n-2)} (\cos n-2)\chi \right\}$$

$$+ \&c.$$

3. The stability of an annulus of rotating fluid is considered for three kinds of disturbances: *fluted*, in which case the ring remains symmetrical about its axis, but its cross-section ceases to be circular; *twisted*, in which case the central circle of the ring is deformed, but the cross-section is undisturbed; *beaded*, in which case the central line remains circular, and the cross-section is a circle, but one of varying radius. It is proved that when the cross-section is small compared with the radius the annular form is stable for fluted and twisted disturbances, but is broken up by beaded waves.

4. In Laplace's proof that Saturn's rings cannot be continuous fluid rotating in relative equilibrium, he assumes that the attraction of the ring at a point on the surface is the same as that of a cylinder. Madame Kowalewski, in her memoir, uses a method which applies only to rings of nearly circular cross-section. Here I have found the potential of a ring of elliptic cross-section. The results are complicated. For a very flat ring of mass M of mean radius c, and whose cross-section has a semi-major axis a, the exhaustion of potential energy is if $(a/c)^2$ is neglected

$$\frac{M^2}{2\pi c} \cdot \left(\log \frac{16c}{a} + \tfrac{1}{4} \right).$$

As applied to Saturn, the result obtained is that for the ring to be continuous fluid its density would have to be about 100 times the density of the planet.

5. Let m be the strength and c the mean radius of a vortex ring. Let its cross-section be given by

$$R = a\{1 + \beta_2 \cos 2\chi + \beta_3 \cos 3\chi + \beta_4 \cos 4\chi + \ldots \}.$$

Then $\beta_2, \beta_3, \beta_4$, &c., are of the 2nd, 3rd, 4th,...., orders in a/c. Their values are obtained as far as $(a/c)^4$.

The velocity of the ring is found to be

$$\frac{\mathrm{M}}{2\pi c}\left\{\log\frac{8c}{a}-\tfrac{1}{4}-\frac{12\log\dfrac{8c}{a}-15}{32}\left(\frac{a}{c}\right)^2\right\}.$$

$$\beta_2 = -\frac{12\,\mathrm{L}-17}{32}\left(\frac{a}{c}\right)^2,$$

&c.

The results agree with those obtained by Mr. Hicks, *viâ* toroidal functions. The fluted oscillations are very briefly discussed.

6. The motion of a number of fine vortex rings on the same axis is discussed. Equations are obtained giving the forward velocity and the rate of increase of the radius for each ring. Let m_1 be the strength, c_1 the mean radius, a_1 the radius of the cross-section, z_1 the distance of the centre of the ring along the axis of z.

It is shown that the kinetic energy of the system is given by

$$\mathrm{T} = 8\Sigma\left\{\frac{m_1^2 c_1}{2}\left(\log\frac{8c_1}{a_1}-\tfrac{7}{4}\right)\right.$$

$$\left. +\,m_1 m_2\int_0^\pi \frac{c_1 c_2 \cos\phi\, d\phi}{\sqrt{\{(z_2-z_1)^2+c_1^2-2\,c_1 c_2 \cos\phi+c_2^2\}}}\right\}.$$

The equations of motion are

$$m_1 c_1 \dot{z}_1 = \frac{1}{8\pi}\cdot\frac{\partial\mathrm{T}}{\partial c}. \qquad\qquad -m_1 c_1 \dot{c}_1 = \frac{1}{8\pi}\cdot\frac{\partial\mathrm{T}}{\partial z_1}.$$

The integral expressing the constancy of momentum takes the simple form

$$\Sigma(m_1 c_1^2) = \text{const.}$$

The following particular cases are worked out in detail:—
1. The motion of a ring following another of equal strength.
2. The direct approach of a ring to a fixed plane.
3. The motion of a ring over a spherical obstacle.

IV. "Analogy of Sound and Colour.—Comparison of the Seven Colours of the Rainbow with the Seven Notes of the Musical Scale, as determined by the Monochord, and of the Wave-lengths of Colour and Sound." By J. D. MACDONALD, M.D., F.R.S. Received March 13, 1893.

Presents, April 20, 1893.

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April 27, 1893.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On the Results of an Examination of the Orientation of a number of Greek Temples, with a view to connect these Angles with the Amplitudes of certain Stars at the time these Temples were founded, and an endeavour to derive therefrom the Dates of their Foundation by consideration of the Changes produced upon the Right Ascension and Declination of the Stars arising from the Precession of the Equinoxes." By F. C. PENROSE, F.R.A.S. Communicated by Professor J. NORMAN LOCKYER, F.R.S. Received December 13, 1892.

(Abstract.)

This investigation is supplementary to Mr. Lockyer's examination of the orientation of the Egyptian temples, in the course of which he has cited passages translated from hieroglyphics, showing most distinctly that there was a connexion between the foundation of those temples and certain stars. He has also shown that the structure of the temples demonstrates that the light from these stars must have been admitted at their rising or setting along the axis of the temples through the doorways, and that in certain temples the doorways have been altered in such a way as to follow the amplitude of the star as it changed, owing to the precession of the Equinoxes, and that in some cases a new temple had been founded alongside of an older one for the same purpose.

Although there does not seem to be any historical or epigraphical record of such a nature in Greece, the architectural evidence is not wanting. On the Acropolis of Athens there are two temples, both dedicated to Minerva, lying within a few yards of one another, both apparently oriented to the Pleiades, the older temple to an earlier position of the star group, and the other to a later one. At Rhamnus there are two temples almost touching one another, both following (and with accordant dates) the shifting places of Spica. In a temple

at Ægina a doorway placed excentrically in the west wall of the cella was adapted for the observation of a setting star.

A clue is given for finding out the dates of the foundations of temples oriented to stars by means of the changes produced upon them by the precession of the Equinoxes, a movement which induces a divergence between the latitudes and longitudes of stars, and their places reckoned in declination and right ascension; so that after the lapse of 200 or 300 years a star which rose or set in the direction of the axis of a temple would have passed to a different amplitude, so as to be no more available for observation, as before, from the adytum.

In the earlier ages of Greek civilisation the only accurate measure of time by night was obtained by the rising or setting of stars, and these were more particularly observed when heliacal, or as near as possible to sunrise. For the purpose of temple worship, which was carried on almost exclusively at sunrise, the priests would naturally be very much dependent for their preparations on the heliacal stars as time warners.

The orientation of temples may be divided into two classes, solar and stellar. In the former the orientation lies within the solstitial limits; in the latter it exceeds them. In Greece there are comparatively few of the latter class.

In the lists of temples which follow, all the orientations were obtained from azimuths taken with a theodolite, either from the Sun or from the planet Venus. In almost every case two or more sights were observed, and occasionally also the performance of the instrument was tested by stars at night. The heights subtended by the visible horizon opposite to the axes of the temples were also observed.

The first list comprises twenty-seven intra-solstitial temples :

7 examples from Athens.			1 example from Sunium.		
3	„	Olympia.	1	„	Corinth.
2	„	Epidaurus.	1	„	Bassæ.
2	„	Rhamnus.	1	„	Ephesus.
2	„	Ægina.	1	„	Platæa.
1	„	Tegea.	1	„	Lycosura.
1	„	Nemea.	1	„	Megalopolis.
1	„	Corfu.	1	„	Argos.

For all these the resulting solar and stellar elements are given, with the approximate dates of foundation, similarly to the following specimen, namely, that of the Temple of Jupiter at Olympia.

Olympia, lat. 37° 38′ N.

Temple of Jupiter.	Orientation angle.		Stellar elements.	Solar elements.	Name of star.
	2̊62 3̊7 4̊6	Amplitude, star or sun	8̊ 38 0̊ N.	7̊ 22 14̊ N.	
		Corresponding altitude	3 0 0 E.	1 42 0 E.	
		Declination	+8 40 0	+6 52 22	
		Hour angles	6ʰ 11ᵐ 37ˢ	7ʰ 34ᵐ 52ˢ	
		Depression of sun. ..	..	14° 12′ 0″	
		Right ascension	23ʰ 40ᵐ 0ˢ	1ʰ 3ᵐ 5ˢ	
		Approximate date ..	B.C. 740	Apr. 6.	

This example has been selected from the rest of the list, because this temple has been chosen for the purpose of showing the method of procedure in working out the elements from the observations, those, namely, of the orientation angle, and of the height of the visible horizon.

A few general remarks, however, seem required respecting the Sun's and star's altitude, and the Sun's depression when the star is to be observed.

For a star to be seen heliacally, it is necessary that the Sun should be just sufficiently below the horizon for the star to be recognised. According to Biot, Ptolemy, speaking of Egypt, has recorded this to be about 11°. But where, as generally in Greece, there are mountains screening the glow which at such times skirts the true horizon, it seems fair at any rate for a first magnitude star to consider 10° as sufficient. I have myself seen Rigel in the same direction as the Sun when elevated 2° 40′ above the sea horizon, the Sun being less than 10° below. Obviously an observer looking from a dark chamber in a well known direction would be more favourably situated.

It is proper to allow about 3° of altitude for a star to be seen above low clouds and the hazy glow which skirts the horizon. The Sun's light, however, seems to be very effective at a lower altitude, and when he appears over a mountain of 2° or 3° altitude the angle may properly be reduced by 20′ or 25′, partly for refraction, and partly because a small segment only of the disk is sufficient for illumination.

The method I have pursued in working out the example of the Temple of Jupiter at Olympia is as follows.

The orientation angle, measured from the south point round by way of west and north, is 262° 37′ 46″, which is equivalent to an amplitude of +7° 22′ 14″. The eastern mountain subtends an angle of 2° 4′. For reasons above given, the solar altitude may be

taken as 1° 42', but that of the star, 3°. Combining these values with the latitude, viz., 37° 38', and using the formula

$$\sin \delta = \cos \text{ zen. dist.} \times \cos \text{ colat} + \sin \text{ zen. dist.} \times \sin \text{ colat} \times \sin \text{ ampl.},$$

we obtain for the star a declination of $+7° 40'$, and for that of the Sun $+6° 52' 22''$. This latter, with the ecliptic obliquity of about 800 years B.C., determines the Sun's right ascension to have been $1^h 3^m 15^s$.

The next step is to enquire if there be any bright star or star group which, at a date consistent with archæological possibility, would have had a declination near to the above-named place, and would also have been heliacal.

Such a star would have required about $6^h 8^m$ to pass from 3° altitude to the meridian, and it would have required to have been about $1\frac{1}{4}^h$ in advance of the Sun to allow it to be seen. The approximate R.A. of such star would therefore be about $23^h 40^m$, and its declination, as already stated, must be about 7° 40' N.

For trials I have used a stereographic projection of the sphere taken on the pole of the ecliptic, but showing also R.A. hours and parallels of declination. Any place on this projection may be chosen and marked on a superimposed sheet of tracing paper, and then if the tracing paper is turned round upon the pole of the ecliptic as a centre, so that the straight line drawn upon it, which in the first instance joined the two poles marked on the projection, is carried round to an angle equal to the amount of precessional movement under consideration, if there be a suitable star marked on the projection the point selected for trial will pass over it or near it, and after the star has been thus roughly pointed out the more exact calculations may be proceeded with. By this process in the case before us the tracing-paper mark coincided almost exactly with the place of α Arietis, and for this star the particulars were carefully computed which have been given in the list of elements.

It should be noticed that there are in every case of intra-solstitial temples four possible solutions of this step. The Sun's amplitude may be due either to the vernal or the autumnal place, and the star might have been heliacal either at its rising or setting. In every instance all these four alternatives have been tried by the preliminary search method, and in every case in temples of old foundation an heliacal star has resulted from one or other of the trials, but never more than one.

The star which has been found as above for the Temple of Jupiter is no other than the brightest star of the first sign of the Zodiac, and therefore peculiarly suited to that god. The same star is connected *with the early* temple of Jupiter Olympius at Athens.

In intra-solstitial temples, by the nature of the case, the stars are

almost entirely confined to the Zodiacal constellations, and conse-
quently suitable stars are very much limited in number.

Another very great limitation arises from the consideration that, to
have been of any service as a time warner, the star must have been
heliacal, and when these two limitations are taken into account, it
becomes improbable to the greatest degree that there should always
have been a suitable star unless it had been so arranged by the
builders of the temple.

In about two-thirds of the cases which I have investigated the
dates deduced from the orientations are clearly earlier than the
architectural remains now visible above the ground. This is ex-
plained by the temples having been rebuilt upon old foundations, as
may be seen in several cases which have been excavated, of which
the archaic Temple of Minerva on the Acropolis of Athens and the
Temple of Jupiter Olympius on a lower site are instances. There
are temples also of a middle epoch, such as the examples at Corinth,
Ægina, and the later temples at Argos and at Olympia (the
Metroum at the last named), of which the orientation dates are quite
consistent with what may be gathered from other sources.

Besides the list of intra-solstitial temples already given, I have
particulars of five for which I have been unable to find an heliacal
star. They are all known to be of recent foundation, when other
methods of measuring time had been discovered. The solar axial
coincidences were no doubt in all these cases connected with the
great festivals of these temples. It was clearly the case in two of
them.

At the Theseum at Athens the date was either October 10 or
March 2. The *Thesea* festival is reckoned to have been on October
8 or 9. For the later Erechtheum the day would have been April 8
or September 3. The great festival of this temple is put down for
September 3.

Leaving the solar temples, we find that the star which was ob-
served at the great Temple of Ceres must have been Sirius, not used,
however, heliacally—although this temple is not extra-solstitial—but
for its own refulgence at midnight. The date so determined is quite
consistent with the probable time of the foundation of the Eleusinian
Mysteries and the time of year when at its rising it would have
crossed the axis at midnight agrees exactly with that of the celebra-
tion of the Great Mysteries.

It is reasonable to suppose that when, as in the case of Sirius at
Eleusis, brilliant stars were observed at night, the effect was
enhanced by the priests by means of polished surfaces.

Herodotus, speaking of a temple at Tyre (B. II, 44), says:—

"Καὶ ἐν αὐτῷ ἦσαν στῆλαι δύο, ἡ μὲν χρυσοῦ ἀπέφθου, ἡ δὲ σμαράγδου
λίθου, λάμποντος τὰς νύκτας μέγαθος."

(Two shafts, one of pure gold, the other of emerald, which shone remarkably at night.)

Of a list of seven extra-solstitial temples which are named, five are more particularly noticed, viz. :—

A temple at Mycenæ and one near Thebes, which are built nearly north and south, but which probably, as was the case at Bassæ, had eastern doorways. The star, α Arietis, which suits the first, seems to point out the dedication of this temple to Jupiter. The other is very remarkable, and connects the Bœotian Thebes with the great Egyptian city; the star was γ Draconis. Thebes was called the City of the Dragon, and tradition records that Cadmus introduced both Phœnician and Egyptian worship. Three of the temples lay more nearly at an angle bisecting the cardinal points; these are Diana Propylæa at Eleusis, a small temple (not yet named) lately discovered at Athens, and the Temple of Venus at Ancona, recovered by means of the walls of a church built upon its traditional site. In these temples the star observed at the first seems to have been Capella, the time of the year when it shone axially at midnight agreeing with that of the celebration of the Little Mysteries, and in the other two the star was Arcturus.

II. "On the Coloration of the Skins of Fishes, especially of Pleuronectidæ." By J. T. Cunningham, M.A. Oxon., Naturalist on the Staff of the Marine Biological Association, and Charles A. MacMunn, M.A., M.D. Communicated by Professor E. Ray Lankester, F.R.S. Received March 6, 1893.

(Abstract.)

In normal specimens of the majority of the Flat Fishes, *i.e.*, of the family Pleuronectidæ, the upper side is pigmented, the lower side opaque white, the colours and markings being characteristic of the species. In symmetrical Fishes which swim vertically the dorsal surface is pigmented, the ventral almost or entirely destitute of pigment. Where the pigment is absent or in small quantity the characteristic silvery brilliancy and iridescence of Fishes' skins is exhibited. It has long been known that the pigment in the skins of Fishes and Amphibia is contained in chromatophores provided with contractile radiating processes, and that the iridescence and brilliant reflection of light is due to special anatomical elements of fixed form, all these elements being placed in the category of connective tissue cells. But exact and detailed descriptions of these coloration elements in *Fishes are not* available. The most complete account of them is that *given by G.* Pouchet in his memoir on the " Changement de Colora-

tion sous l'Influence des Nerfs" ('Journ. Anat. et Phys.,' 1876). He there gives the separate reflecting cells the convenient name "iridocytes," and refers to the silvery layer of the skin under the name "argenture." The anatomical analysis of the structural coloration elements having thus not previously been adequately carried out, we have described these elements as they are found in Pleuronectidæ and various other Fishes. In the former family there are two kinds of chromatophores, the black and the coloured, the latter usually of some shade of yellow or orange. The coloured elements in the skin on the upper side are chiefly developed in the more superficial layer immediately beneath the epidermis and for the most part outside the scales, and on the inner side of the skin in the subcutaneous tissue, the rest of the skin being almost destitute of these elements. In the superficial layer the iridocytes are somewhat polygonal plates of irregular shape, distributed uniformly, and separated by small inter-spaces. The chromatophores are much larger, and farther apart, and are superficial to the iridocytes, although sections show that their processes often pass down between adjacent iridocytes. The coloured chromatophores have less definite outlines than the black, and as a rule radiating processes are but indistinctly indicated in them. The external part of the coloured chromatophore consists of diffused yellow pigment, while in the centre the concentration of the pigment produces a deeper colour, varying from orange to red, as in the Plaice and Flounder. On the upper side of the Fish the subcutaneous coloration elements are quite similar, but not so uniformly distributed; the iridocytes are larger, and the chromatophores not so symmetrical in shape.

The lower side of the normal Flounder is uniformly opaque white, like chalk. Here in the more superficial part of the skin there is a uniform layer of iridocytes like those of the upper side, opaque and reflecting, but not very silvery or iridescent. Chromatophores are entirely absent. In the subcutaneous layer there is a continuous deposit of reflecting tissue, to which the whiteness of the skin is due, the superficial iridocytes not being sufficiently thick to make the skin so opaque. We have shown by comparing the adults of different species, and stages of growth of the same species (Flounder), that the subcutaneous deposit is merely a further development of a layer of separate iridocytes which enlarge until they become continuous. In some cases, *e.g.*, *Arnoglossus*, the subcutaneous layer of the lower side remains permanently as a layer of separate plates or iridocytes. The continuous deposit in all respects corresponds to the subcutaneous tissue to which is due the silvery glitter of the Mackerel or Herring, and we have called it the argenteum.

We have shown by descriptions of the coloration elements in a number *of species of* symmetrical Fishes such as Mackerel, Whiting,

Gurnard, *Cottus*, Pipe-fishes, &c., that the general distribution of the elements is constant in all, the differences being in minute details. Thus, there is always an argenteum consisting of reflecting tissue composed of needles or granules in a layer of greater or less thickness. And in the superficial region of the skin there is a layer of chromatophores associated with a thinner deposit of reflecting tissue corresponding to the iridocytes of the Pleuronectid. Thus, in the Herring the superficial reflecting tissue is in the form of a layer o slender rods or prisms lying side by side and adhering to the inner surface of each scale, forming a coating to the latter when it is removed, and endowing it with its beautiful iridescence. The scales themselves are never silvery or iridescent. In the Herring the argenteum consists of similar rods in close apposition, forming laminæ. On the dorsal surface of the body, where the argenteum becomes thinner, chromatophores are found on its surface and penetrating it with their processes; on the ventral surface these are absent, and the argenteum is very thick.

The ultimate histological relations of the coloration elements we have not completely elucidated, but have merely described the relations as seen in sections, pointing out the difficulty of regarding the iridocytes of the Pleuronectids as cells, considering their homology with the reflecting tissue in other forms, such as the Herring, and the fact of the argenteum, a continuous deposit whose cellular nature is very improbable, being developed from iridocytes.

In chemical and physical properties the substances contained in the coloration elements are as distinct as the elements are in appearance and form. The black chromatophores owe their colour to a melanin which is granular in its natural condition, is a nitrogenous body, and is very refractory towards reagents. The pigment of the coloured chromatophores is always a lipochrome, and the absorption bands of the various lipochromes obtained from the Fishes examined do not differ to any great degree. The reflecting tissue was found always to consist of guanin in the pure state, not, as has often been stated, to a combination of guanin and calcium. The differences in the appearance of the reflecting tissue in the natural state, whether it is silvery, chalk white, or iridescent, depend on the form of its minute elements. It is chalk white when granular, silvery when composed of very fine needles in a thick layer, iridescent when composed of thicker prisms in a thin layer. The opacity and reflecting power is a property of the guanin itself in any form. Besides these substances, large crystals of phosphate of calcium were found in many skins, both of Pleuronectidæ and other Fishes, and from these or from the scales has probably been derived the calcium supposed to *be associated* with the guanin.

These investigations of the elements and substances of coloration

were undertaken in order to find out what exactly took place when coloration was developed in the lower side of Flounders in certain experiments carried on at the Plymouth Laboratory since the spring of 1890.   The first of these experiments was described in the 'Zoologischer Anzeiger,' No. 354, 1891.   The method of the experiments is to take young Flounders in process of metamorphosis, or at a later stage, and keep them in a vessel with a glass bottom without gravel or sand, and then to direct light from a window on to the lower sides by means of an inclined mirror placed beneath the vessel and opposite the window.   The first experiment was not quite conclusive, although some pigment was found on the lower sides of the fish after an exposure to light of four months.   The second experiment was quite conclusive.   Four Flounders were taken on September 17, 1890, from a number reared in the aquarium since the preceding May : they were five to six months old, and 5 to 8 cm. in length.   They had been living under ordinary conditions, and were in all respects normal, having no colour on the lower sides.   They were placed in the vessel above the mirror.   On one of these, two faint specks of pigment were observed on April 26, 1891, one died on the following July 1, which showed no pigment, and one on September 26, 1891.   The latter was 16·7 cm. long and showed only a little pigment on the posterior part of the operculum.   At this time one of the two survivors had developed pigment all over the external regions of the lower side, and the other had a few small spots.   The first of these two is still alive (March, 1893), being now three years old, and it is now pigmented over the whole of the lower side except small areas on the head and the base of the tail.   A drawing showing its condition in November, 1891, was exhibited at the soirée of the Royal Society in 1892, and is laid before the Society with this paper.   The other specimen died on July 4, 1892.   It was then 25 cm. long and had a good deal of pigment in scattered spots on the lower side.   This specimen had been exposed about one year and ten months.   Several other experiments gave similar results.   The Flounders which were reared in the aquarium under ordinary conditions showed no such tendency to develop pigment; a few, as in nature, were found to have small patches of pigment on the lower side, but the percentage was extremely small, and the pigment in such cases was constant, not progressive, as in the fish exposed to light.

The occurrence of abnormal coloration in Pleuronectids is fully considered in the memoir; a large number of specimens are described, and it is shown that there is no evidence whatever that these specimens have been exposed to abnormal conditions.   We conclude that these abnormalities are congenital and not acquired.

We find that where pigment is developed on the lower side, whether in the experiments or as a congenital abnormality, the argument is

much diminished, as it normally is on the upper side, and it is suggested that possibly there is an inverse physiological relation between the formation of guanin and the formation of pigment.

We conclude that exposure to light does actually cause the development of pigment in the form of normal chromatophores on the lower side of the Flounder, and also causes the absorption of the argenteum to a great extent. We infer, in spite of the occurrence of congenital abnormalities, that the exclusion of the light from the lower sides of Flat Fishes is the cause of the absence of pigment from that side in normal specimens. We think that the fact that the metamorphosis of the Flounder takes place at first normally, in spite of the light coming from below and being shut off from above, is, in respect of the pigmentation, in favour of the inheritance of acquired characters. When the exposure is continued long enough, the change that has taken place in consequence of heredity is reversed, and pigment appears.

We have discussed briefly the question of the physiological process of the formation of the pigment, but we have at present no decisive result to offer in this part of the subject, and need not include it in this abstract.

We consider that these investigations afford support to the view that the incidence of light is the reason why the upper and dorsal surface of animals is more strongly pigmented than the lower or ventral throughout the animal kingdom, and that the absence of light is the cause of the disappearance of pigment in many cave-inhabiting and subterranean animals.

III. "The Electric Organ of the Skate : Note on an Electric Centre in the Spinal Cord." By J. C. EWART, M.D., Regius Professor of Natural History, University of Edinburgh. Communicated by Professor Sir W. TURNER, F.R.S. Received March 15, 1893.

Having considered the development and structure of the electric organ of the Skate, it appeared to me desirable, by way of making my work more complete, to reinvestigate the nervous apparatus of the organ, and more especially to ascertain whether, as in *Torpedo* and *Gymnotus*, there is an electric centre. In *Torpedo* the electric organs are developed from a limited number of myotomes, and innervated by afferent fibres, belonging to a limited number of cranial nerves, which proceed from two large collections of cells—the electric *lobes*—situated in the region of the medulla. In *Gymnotus* the nerves *for the electric* organs proceed from two well-marked cellular tracts

which extend along the greater length of the spinal cord, one at each side of the central canal. In the case of the Skate the question at the outset is, granting the existence of an electric centre, is it, as in *Torpedo*, situated in the brain or, as in *Gymnotus*, in the spinal cord ? Sanderson and Gotch* made out that in the Skate "a reflex centre is situated in the optic lobes," but, notwithstanding this, these lobes in the Skate in no way differ histologically from the corresponding structures in *Acanthias* and other Selachians unprovided with electrical organs.

The development of the Skate's organ from portions of the caudal myotomes, and its innervation by afferent fibres from certain caudal nerve, point to the electric centre being situated in the spinal cord rather than in the brain, and to its being, as in *Gymnotus*, on a level, and all but co-extensive, with the electric organ.

Having observed, when working at the development of the electric organ, a number of large nerve-cells in the caudal portion of the spinal cord, the sections of Skate embryos made some years ago were first examined. It soon became evident that in sections from the middle of the tail on a level with the electric organ certain cells of the anterior horn of the cord were very much larger than in sections through the root of the tail, and further that in late embryos and very young Skate there was an electric centre, resembling in many respects the electric centre in *Gymnotus*.

It did not, of course, follow that the electric nerve-cells persisted into adult life. They might degenerate, and thus the supposed feebleness of the Skate's organ might be accounted for. The fact that the Skate's organ increases in size as the fish grows larger led me, however, to expect that large nerve-cells would be found in the caudal region of the spinal cord in well-grown fish. In this I was not disappointed, for, though there was at first some difficulty in demonstrating the presence of electric nerve-cells in large fish, on obtaining perfectly fresh material their position, size, and relations were easily made out, and the remarkable difference in the appearance of sections of the cord at, and in front of, the root of the tail, from sections on a level with the electric organ, was at once evident. From the observations already made, it appears that the electric centre in the Skate closely resembles, from a morphological point of view at least, the electric centre in *Gymnotus*. The electric tract is, however, much shorter in the Skate than in the Electric Eel, and the cells are relatively fewer in number. On the other hand, the cells in the Skate are larger than in *Gymnotus*, and this is true not only of *Raia batis* but also of *R. radiata*, in which the organ is extremely small and poorly developed. Nerve-cells from the electric centres of *Torpedo, Gymnotus,* and *Raia* are represented in the accompanying

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figures.   Fig. 1 represents a cell from the electric centre of the Skate
(a *R. batis* two feet in length) ; fig. 2 a cell from the electric centre
of a well-grown *Gymnotus;* and fig. 3 a cell from the electric lobe of a
large *Torpedo.*   All three cells are camera drawings, and the same
lenses were used in each case—objective D and ocular 2, Zeiss.   It will
be noted that, though the cell from the Skate is much smaller than
the *Torpedo* cell, it is decidedly larger than the one from *Gymnotus.*

   In sections of the Skate's cord on a level with the electric organ,
small, as well as large, cells are usually visible in the anterior horn.
The small cells are in connexion with the fibres which supply the un-
transformed caudal muscles.   They agree exactly with the cells in
the anterior horn throughout the entire length of the spinal cord
lying in front of the electric organ region.   One of these unenlarged

3. Torpedo.

2. Gymnotus.

1. Raia.

5.

4.

J.C.E.
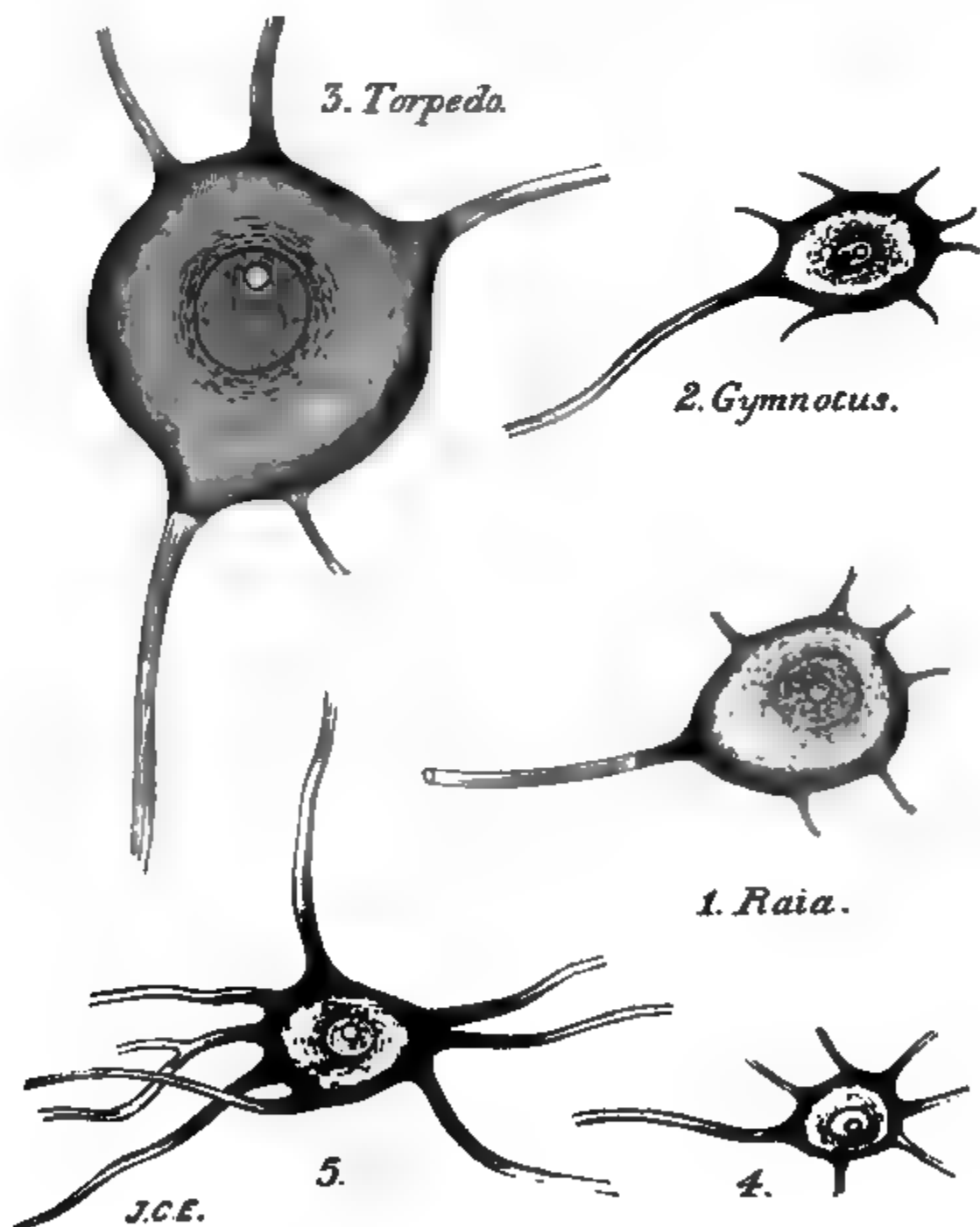
motor cells is represented in fig. 4. It was drawn from a section of the cord (of the same fish from which fig. 1 was taken), about six inches in front of the electric organ. It closely resembles, except in size, the electric cell (fig. 1), and it also resembles the large motor cells of the Mammalian cord. A motor cell from the spinal cord of a Mammal, drawn to the same scale as the other cells given, is represented in fig. 5.* This cell, smaller than the electric cell of the Skate (1), and still smaller than the cell from *Torpedo* (3), is about the same size as the electric cell of *Gymnotus* (2).

With the help of sections through a series of embryo Skate, for most of which I was indebted to Dr. Beard, I have been able to study the development of the cells in the Skate's electric centre. This part of the subject, together with the condition of the electric cells in large fish, will be dealt with in a subsequent communication. It may, however, be stated now : 1. That in *R. batis* embryos under 5 cm. in length none of the motor cells in the caudal region have undergone enlargement. 2. That in an embryo 5·8 cm. in length, although the muscular fibres seemed still unchanged, certain cells in the anterior horn of the caudal portion of the cord were distinctly larger than similarly shaped cells in their vicinity. 3. That in an embryo 15·5 cm. in length, in which the electrical elements were already well developed, the electric nerve-cells were large and conspicuous, so that sections through the cord in the region of the electric organ presented quite a different appearance from sections through the root of the tail, where no change had taken place in the cells of the anterior horn.

Presents, April 27, 1893.

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* For the use of the section from which fig. 5 was drawn I am indebted to Sir William Turner, F.R.S.

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May 4, 1893.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

A List of the Presents received was laid on the table, and thanks
ordered for them.

In pursuance of the Statutes, the names of the Candidates recom-
mended for election into the Society were read from the Chair as
follows :—

Burnside, Professor William, M.A.
Dunstan, Professor Wyndham R.,
   M.A.
Ellis, William, F.R.A.S.
Ewart, Professor J. Cossar, M.D.
Gairdner,   Professor   William
   Tennant, M.D.
Hobson, Ernest William, D.Sc.
Howorth,   Sir   Henry   Hoyle,
   K.C.I.E.
Newton, Edwin Tulley, F.G.S.

Sherrington, Charles Scott, M.B.
Stirling, Edward C., M.D.
Thornycroft, John Isaac, M.Inst.
   C.E.
Trail, Professor James William
   Helenus, M.D.
Wallace, Alfred Russel, LL.D.
Worthington,   Professor   Arthur
   Mason, M.A.
Young, Professor Sydney, D.Sc.

The following Papers were read :—

I. " On the Thickness and Electrical Resistance of Thin Liquid
   Films."   By A. W. REINOLD, M.A., F.R.S., Professor of
   Physics in the Royal Naval College, Greenwich, and A. W.
   RÜCKER, M.A., F.R.S., Professor of Physics in the Royal
   College of Science, London.   Received March 10, 1893.

(Abstract.)

*The paper gives an account of experiments made for the purpose
of determining* the thickness of black soap films formed of solutions

of varying composition.   Two methods of experiment were employed:
(1) an optical method, in which the mean thickness of about 50 plane
black films contained in a tube was deduced from observations of in-
terference phenomena; and (2) an electrical method, in which the
thickness of a cylindrical black film was derived from a measurement
of its electrical resistance.   The optical method involves the assump-
tion that the refractive index of a thin film of liquid is the same as
that of a large quantity of the same liquid.

Reasons are given for the belief that the refractive indices in
question, if not identical, differ only slightly, and hence that the
thickness of a film as determined by the optical method is the true
thickness.

In the electrical method the assumption is made that the specific
conductivity of a liquid does not alter when the liquid is drawn out
into a thin film.

If the results obtained by the two methods agree, the conclusion is
that the specific resistance of a film is not affected by its tenuity; if
they differ widely from each other, a change in the specific conduc-
tivity of the liquid must have taken place.

The authors showed, in 1883, that for a solution of hard soap con-
taining 3 per cent. of KNO_3, with or without the admixture of
glycerine, the mean thicknesses of black films, as measured by each
of the two methods, were in close agreement.   For such solutions,
then, the specific conductivity is the same whether the liquid be
examined in considerable bulk or in the form of a film 12 $\mu\mu$ in thick-
ness.   The accuracy of this result has been confirmed by a large
number of observations made during the last three years.

If the proportion of KNO_3 added to the solution be diminished, the
thickness of a black film, whether measured optically or electrically,
is found to undergo a change.

I. *Optical Method.*

The following table shows the change in the (true) thickness of a
black film due to a change in the quantity of dissolved salt.

1 part of Hard Soap in 40 of Water.

Percentage of KNO_3..........	3	1	0·5	0
Mean (true) thickness of black film in $\mu\mu$.................	12·4	13·5	14·5	22·1

Experiments made with soft soap and with solutions containing
glycerine confirm these results.

2 F 2

The change in the mean thickness of a black film due to variation in the percentage of dissolved soap is shown in the following table:—

Hard Soap.   No dissolved Salt.

Proportion of water to soap....	1/30	1/40	1/60	1/80
Mean thickness of black film in $\mu\mu$....................	21·6	22·1	27·7	29·3

When the solution contains 3 per cent. of KNO_3 variation in the proportion of dissolved soap has little influence on the thickness of a black film, as is evident from the following numbers:—

Hard Soap.   3 per cent. KNO_3.

Proportion of soap to water....	1/40	1/50	1/60	1/70
Mean thickness of black film...	13·0	12·1	11·55	12·1

II. *Electrical Method.*

It has been stated that for a soap solution containing 3 per cent. of KNO_3 the thickness of a black film as measured electrically is practically the same as that measured optically. If, however, the proportion of KNO_3 be diminished, the thickness (measured electrically) increased in a far larger ratio than would be inferred from the optical method. If the proportion of salt be diminished to zero, the thicknesses thus calculated are much greater than the greatest thickness at which a film can appear black. In such cases, therefore, the electrical method does not give the true thickness of the black, and the hypothesis that the specific conductivity of the film and of the liquid in mass are identical is untenable.

The following table shows the change in apparent thickness due to diminution in the quantity of dissolved salt:—

Hard Soap.

Percentage of KNO_3	3	2	1	0·5	0
Mean apparent thickness of black film (measured electrically)	10·6	12·7	24·4	26·5	154

The large value obtained for the apparent thickness in the case of

the unsalted hard soap solution is confirmed by experiments on a solution of unsalted soft soap, which gave a mean appearent thickness of 162 $\mu\mu$.

In different films the measured thicknesses of the black differ widely from each other, the limits being roughly 80 $\mu\mu$ and 230 $\mu\mu$. This large variation is due in some cases, at all events, to a real variation in the thickness. Two different shades of black are (in cases where the solution contains little or no salt) frequently seen in a film. They are separated from each other by a line of discontinuity which is irregular in shape. Comparative measurements on the two shades of black are difficult to make, as the regions they occupy are rarely sufficiently extended or separated by a line sufficiently approximating to a horizontal circle for the application of the method of measurement which the authors employ. Measurements, however, have been made, and the results indicate that the electrical thicknesses of the two kinds of black are approximately as 2 : 1.

Details are given in the paper of numerous experiments carried out with the object of determining the *cause of the great increase in electrical conductivity in black films made from unsalted soap solutions.*

The results have shown that the increase of specific conductivity in question—

1. Is independent of moderate changes of temperature.

2. Is not due to the absorption or evaporation of water by the film.

3. Is not due to change in the composition of the liquid by electrolytic decomposition produced by the current used to measure the electrical resistance of the film.

4. Is not affected by a very large change in the quantity of CO_2 in the air around the film.

5. Is practically unaltered if the films are formed in an atmosphere of oxygen.

The next question to be answered was whether the large changes in specific conductivity affect black films only, or whether similar phenomena can be detected in the case of thicker films.

The conclusions arrived at were (1) that the specific conductivity of a film increases as the thickness decreases and (2) that this increase is less in the case of a film to which a salt has been added and is *nil* when the proportion of salt is as much as 3 per cent. The following figures illustrate the first of these conclusions :—

Hard Soap 1/60.

Optical, *i.e.*, true, thickness of film in $\mu\mu$	641	296	97	27·7
Ratio of electrical to optical thickness	1·66	1·98	4·47	5·8

In the case of a soap solution containing 3 per cent. of KNO_3 the results of the electrical and optical methods of measurement agree for thicknesses greater than 450 $\mu\mu$. At thicknesses between 450 and 200 $\mu\mu$ the ratio is generally above unity, being in some cases as large as 1·28, but there is no clear indication that its value increases as the film thins, and when the thickness corresponding to the black is reached the ratio is again unity.

The paper concludes with a discussion as to the cause of the increase of electrical conductivity in thin films. The authors point out that it may be attributed either to a modification of the chemical constitution of the film brought about by its tenuity, or to the formation of a pellicle on the surface. They prove that the experimental results cannot be explained by the formation of a pellicle only, but that they are consistent either with the former or with a combination of both causes. To discriminate between them it will be necessary to carry out observations in gases other than air, and an apparatus specially designed for this purpose is being constructed.

II. "Organic Oximides: a Research on their Pharmacology." By H. W. POMFRET, M.D., F.R.C.S., late Berkeley Fellow at the Owens College. Communicated by Sir WM. ROBERTS, F.R.S. Received March 6, 1893.

(Abstract.)

Organic oximides may be concisely defined as "bodies containing the chemical group $=N\cdot OH$ attached to a carbon atom."

These bodies may be broadly divided into two classes: (*a*) Those whose preparation involves the use of hydroxylamine; these are known as "oximes," whence the generic name "oximide" is derived. (*b*) Those which are prepared independently of hydroxylamine. These latter may be obtained by the aid of nitrous acid, and have been termed "isonitroso-" bodies. This group $=N\cdot OH$ must be distinguished from the true "nitroso-" group $-NO$. The oxime group is bivalent, being regarded as a compound of trivalent nitrogen with the monovalent radical hydroxyl. The true nitroso-group is monovalent, two "affinities" of the nitrogen being taken up by oxygen.

There is furthermore an essential structural difference between the bodies forming the subject of this research and the nitrites. In the nitrites the nitrogen is attached to oxygen, whereas in these oximido-bodies the nitrogen of the oxime group is attached to carbon, and the larger group "$C=N\cdot OH$" may be considered present.

For example

$$
\begin{array}{cc}
\mathrm{CH_3} & \mathrm{CH_3} \\
\mid & \mid \\
\mathrm{CH} & \mathrm{CH_2} \\
\parallel & \parallel \\
\mathrm{N\cdot OH} & \mathrm{O\cdot NO}
\end{array}
$$

<div align="center">Ethylaldoxime.   Ethyl nitrite.</div>

It is this larger group, $C{=}N{\cdot}OH$, which has been called the "oximido-" group.

The ultimate object of this research was to correlate this structural relationship with the pharmacology of these bodies, and to discover how far they may possess any pharmacological type which can be isolated and referred to the presence of the group ${=}N{\cdot}OH$.

Representative members were selected for pharmacological investigation from several series of the oximido-bodies. From the fatty aldoximes were taken—ethylaldoxime, propylaldoxime, isobutyl-aldoxime and œnanthaldoxime; acetoxime was chosen to represent the ketoximes; isonitrosoacetone to represent the isonitrosoketones; benzaldoxime and salicylaldoxime to represent the aromatic bodies. I had previously investigated the pharmacology of quinonoxime (paranitrosophenol, $C_6H_4{\cdot}O{\cdot}NOH$).

It may be said at once that the physiological actions of these substances recall in many points the properties of nitrites. When oxidised at the body temperature decomposition takes place, and very soon the presence of a nitrite can be demonstrated.

This decomposition consists essentially in the separation of hydroxylamine from an aldehyde or ketone. In other cases the intermediate production of hydroxylamine has not been proved, but an immediate oxidation of the whole molecule and severance of nitrous acid has appeared possible.

Before analysing the experimental results of the actions of these oximido-bodies, it was necessary to define the actions of their corresponding aldehydes and ketones.

<div align="center">

Ethylaldehyde, $CH_3{\cdot}COH$.

Propylaldehyde, $CH_3{\cdot}CH_2{\cdot}COH$.

Isobutylaldehyde, $(CH_3)_2{\cdot}CH{\cdot}COH$.

Heptylicaldehyde, $C_6H_{13}{\cdot}COH$.

</div>

The action of these fatty aldehydes on voluntary muscle is chiefly evidenced in two ways—contracture and loss of irritability and contractility. A primary stimulation is always seen in observations with minimal stimuli, but becomes more and more transient in equivalent dilutions as the series of aldehydes is ascended.

In muscle tracings a primary increased range of contraction is

seen when dilute solutions are used; but the dilution must be increased with the atomic weight of the aldehyde.

There is a primary shortening of the latent period which also varies as the primary stimulation. This same action is also reflected in the muscle curves, where the abrupt ascent and increased height, whilst always to be seen as initiatory effects, are found to become more transient. At the same time the descending arm of the curve always shows the rigidity of contracture, and that in increasing degree.

As the group of aldehydes is ascended, muscle-nerve preparations show a gradually increasing loss of irritability in the nervous path, both absolutely and also slightly in comparison with a similar loss of irritability in the muscle itself.

A primary slight exaltation of irritability in the nervous path also becomes a little more evident, and, since the nerve trunks have shown no such action, the nerve endings must be the seat of such primary stimulation.

In their action on the spinal cord these fatty aldehydes produce a primary increase of irritability followed by a secondary depression. The intensity of this primary stimulation of the spinal cord seems scarcely to vary in the case of the lower three members, that is to say, ethyl-, propyl-, and isobutyl-aldehyde, whereas the potency of their secondary depressant action intensifies with their increasing weight. Œnanthol causes a more marked primary stimulation of the cord than would inferentially be expected, and may cause reflex convulsions in the frog.

Ethyl-, propyl-, and isobutyl-aldehyde all constrict the vessels of the excised sheep kidney. This action varies inversely as the atomic weight; thus, isobutylaldehyde constricts the vessels less than propylaldehyde and the latter less than the ethyl compound. Œnanthol first constricts and then dilates the same vessels. In the tortoise ethylaldehyde and propylaldehyde are again vaso-constrictors. The effect of pithing the cord in the tortoise has shown that both ethyl- and propyl-aldehyde have a local constrictory influence on the vessel walls, and it would appear that this action is reinforced during the period of exalted sensibility of the cord by a like influence exerted through the spinal centres. As the cord becomes depressed this central constricting influence is abolished, or even replaced by an influence antagonistic to the local constrictory action.

Isobutylaldehyde dilates the tortoise's vessels slightly, and the action increases somewhat as the circulation is continued. This vaso-dilating action of isobutylaldehyde is again exerted locally on the walls of the vessels, and is at first either uninfluenced by, or is slightly antagonised by, the spinal cord; later, a vaso-dilating influence is also exerted through the spinal centres.

Œnanthol also dilates the vessels of the tortoise, but, provided the spinal cord be intact, by no means so markedly as the vessels of the excised kidney. Pithing the cord balances the comparison, for then the tortoise's vessels are dilated by œnanthol to as great a degree as those of the excised kidney. Œnanthol has, therefore, its local vaso-dilating action inhibited by a central action exerted through the spinal cord.

All these fatty aldehydes have essentially the same action on the heart, the difference between them being simply one of degree. They all tend to slow the cardiac rhythm, and have a primary tonic and secondary depressant action. As the atomic weight of the aldehyde increases, weaker solutions are required to show the tonic effect, and the arrest in diastole is more quickly reached.

Ethylaldoxime, $CH_3 \cdot CH:NOH.$
Propylaldoxime, $C_3H_6:NOH.$
Isobutylaldoxime, $C_4H_8:NOH.$
Œnanthaldoxime, $C_7H_{14}:NOH.$

Contrasting now the actions of the fatty aldehydes with the actions of their corresponding aldoximes as observed by similar experimentations, it is seen how closely these latter bodies reflect the combined actions of a nitrite and aldehyde.

These fatty aldoximes depress the irritability of voluntary muscle. This depressant action is also possessed by nitrites and by aldehyde. Ethylaldehyde has also a slight primary stimulant action, but this in the aldoxime molecule is for the most part absent, being counteracted by the oxime group.

The aldoximes diminish the extensibility, the elasticity, and the range of contraction of voluntary muscle, actions also possessed both by aldehyde and nitrite.

Ethylaldoxime has rarely produced an initial increased range of contraction, which initial increase is a usual effect of ethylaldehyde, but might be annulled by nitrites.

The latent period is not affected by nitrites. Under the influence of both aldehydes and aldoximes there is a primary shortening of this period.

Again, these fatty aldoximes differ amongst themselves in their action on voluntary muscle exactly as do the corresponding aldehydes. As the series is ascended the action on voluntary muscle becomes more toxic, as seen firstly in the increasing degree of contracture, and secondly in the more rapid loss of irritability.

In their action on the spinal cord they present the same characters and variations as those possessed by the corresponding aldehydes. Comparison simply shows a more marked primary stimulation in the

case of the aldehydes. This might be explained by the fact of nitrites being purely depressant to the cord. The stimulant action of the aldehyde would in this sense be discounted by the $=$N·OH group of the aldoxime.

In the same way the experimental circulation of these aldoximes has shown their action to be that of the corresponding aldehyde modified by a local vaso-dilating influence.

Lastly, accelerated rhythm coupled with depression is the type of the cardiac action of all but very dilute solutions of nitrites, and perfusion experiments on the heart have shown that these aldoximes possess this nitrite type superadded to the action of the corresponding aldehydes. Accelerated rhythm is invariably produced, whilst the primary tonic effect is less marked.

Benzaldehyde, C₆H₅·COH.
Salicylaldehyde, OH·C₆H₄·COH.

These aromatic aldehydes have the same type of action as their fatty homologues, but differ from them in their much more powerful toxicity and greater dominance of irritation, which is more especially seen in their action on the spinal cord and voluntary muscle.

They are both vaso-dilators, though such action is not very pronounced.

A solution of either of these aldehydes not stronger than 1 part in 30,000 parts has a marked action on the frog's heart. The rhythm is slowed whilst the amplitude of beat gradually diminishes, and the heart becomes arrested in diastole. Stronger solutions cause imperfect dilatation of the ventricle during diastole, with final arrest in systole.

Benzaldoxime, C₆H₅·CH:NOH.

Salicylaldoxime, $C_6H_4 \big< {}^{OH}_{CH:NOH}$.

These two aromatic aldoximes have been found to scarcely differ in their physiological actions from their corresponding aldehydes. The powerful aldehyde influence almost completely obscures any action which might be attributable to the NOH group in their structure.

Progressive contracture and loss of irritability are the main features of their action on voluntary muscle. A primary stimulation of voluntary muscle, doubtful in the actions of the fatty aldoximes, is well in evidence in the case of those aromatic compounds, and in muscle-nerve preparations is seen equally well, should the stimulus have been thrown along the nerve or directly into the muscle fibres.

The results of subcutaneous injections of these two aldoximes have

indicated their action on the spinal cord to be paramount. The reflex irritability of the cord is greatly increased until the muscles of the limbs are thrown into tetanic convulsions.

The presence of the NOH group is, however, felt by the vessel walls of the excised kidney, and is revealed by the much more powerful vaso-dilating action exerted by salicylaldoxime than by salicylaldehyde.

Also in the action of these two aldoximes on the heart the influence of the NOH group is probably to be traced in the fact that no retardation of rhythm occurs as is seen in perfusion experiments with the aromatic aldehydes, though in all other respects the cardiac actions of these two classes of aromatics are identical.

Acetoxime, $(CH_3)_2:C:N·OH.$
Isonitrosoacetone, $CH_3·CO·CH:N·OH.$

I have investigated the physiological actions of these substances by the same methods as those employed for the previously described bodies, and have found their actions to closely resemble those of the fatty aldoximes. The structural difference is not borne out pharmacologically. Acetoxime more especially repeats the actions of propylaldoxime, and in the presence of this fact it is interesting to observe that the molecular weight of acetoxime is exactly equivalent to that of propylaldoxime.

Isonitrosoacetone finds its parallel intermediate to propylaldoxime and isobutylaldoxime, in some of its actions approaching the former, but, on the whole, being nearer to the latter.

In molecular weight isonitrosoacetone finds its exact equivalent in isobutylaldoxime.

Acetone, $CH_3·CO·CH_3.$

I have examined the actions of acetone on the isolated tissues and organs, and have found that, except in the case of voluntary muscle, these actions differ in little from those of propylaldehyde. Nervous depression is the cardinal feature of the general action of acetone on the frog. Injections have paralysed the spinal cord. In muscle-nerve preparations acetone quickly depresses the irritability of the nervous path.

It is in its action on voluntary muscle that acetone diverges most from the aldehydes. Pure acetone causes no contracture in muscle, and the muscle irritability is depressed rather than the contractility. In fact, the action of acetone on voluntary muscle I have found to closely resemble that of ethyl alcohol. ·

On the vessels of the tortoise and excised sheep's kidney, acetone has not been found to possess any action, beyond at times an equivocal constriction.

Acetone is almost innocuous to the frog's heart in all but very strong doses, when the only action is depressed systole with final arrest in diastole.

Seeing, therefore, the resemblance in action found to exist between a ketoxime and an aldoxime, and also between isonitrosoacetone and an aldoxime; seeing, further, the resemblance in action between the involved aldehydes and ketone, it must follow, as a corollary, that the influence of the oxime group must in each case be the same. This influence is that of a nitrite, as was also found to be the case in the aromatic aldoximes.

The only discrepancy arises in the actions of acetoxime and of iso-nitrosoacetone on voluntary muscle. They both give rise, when present in strong solution, to the development of some contracture, a phenomenon which cannot be ascribed to acetone.

During the course of this research it has been sought to explain the nature of muscle contracture, and it has been determined that the phenomenon is probably due to direct irritation of the nerve end plates, the irritant in the case of these oximido-bodies being an aldehyde, or, perhaps, more accurately, the COH group.

In support of this contention several facts may be here adduced.

It is an active process associated with an increased formation of heat.

Tracings show the onset and decline of contracture to be in relationship with the shortening and lengthening of the latent period.

The development of contracture is prevented by curare.

A primary increase of irritability in the nervous path of muscle-nerve preparations can be traced to the end plates.

This irritability, better expressed as exalted conductivity of the end plates, becomes more marked as the power of the aldehydes to cause contracture increases.

The decline of contracture is synchronous in its onset with the loss of conductivity through the end plates.

Experiments on the oxidation of acetoxime and isonitrosoacetone have led to the detection of an aldehyde—pyroracemic aldehyde, in the case of acetoxime, and acetylformic aldehyde in the case of iso-nitrosoacetone. This formation of aldehyde, should it take place in the tissues, would then be a sufficient explanation for their giving rise to contracture. On the other hand, it might be argued that the oxime group, whilst in all other respects giving rise to actions identical with those of nitrites, yet exerts a primary stimulant action on nerve centres and on the muscle end plates. Such an action this investigation has not disproved.

III. "On the alleged Increase of Cancer." By George King, F.I.A., F.F.A., and Arthur Newsholme, M.D., M.R.C.P. Communicated by Dr. J. S. Bristowe, F.R.S. Received February 27, 1893.

(Abstract.)

Attention is first drawn to the alarming increase in mortality from cancer, shown by the Registrar-General's figures, and to the fact that the view that this increase is due to more accurate diagnosis and certification has been partially abandoned.

An attempt is then made to test this conclusion, by a study from an independent standpoint of the official cancer death-rates for England and Wales, Scotland, and Ireland; and by a comparison of these death-rates with other data obtained from the experience of the Scottish Widows' Fund Life Assurance Society, and from the official cancer returns for the city of Frankfort-on-the-Main.

In order to make the figures from these different sources exactly comparable, corrections have been made for variations in age and sex distribution. A standard population is taken (that of the "English Life Table, No. 3. Persons.") The death-rates at the different age-groups in each case are then multiplied into the populations at the corresponding age-groups in the standard population assumed as a common basis. Thus we obtain in each case the total deaths from cancer per annum among a million persons aged 25 and upwards, grouped as in the standard population, and can contrast the different totals obtained, without any fallacy arising from varying age and sex distribution of population.

The results obtained are grouped in septennial periods, as the figures relating to the Scottish Widows' Fund Assurance Society were only obtainable in septennial periods. From these septennial results, the corresponding death-rates are obtained for each single year by an application of the graphic method employed by Milne in the construction of his Carlisle Life Table. These are shown as a series of curves.

The Irish curves are the lowest, probably because medical attendance in Ireland owing to poverty is on the average more meagre than in Great Britain. The English curves for males and females are very far apart. The Scottish curves for the two sexes are nearer together than the English, the apparent cancer mortality in Scotland for males being higher and for females lower than in England. The greater propinquity of the Scotch male and female curves may be ascribed to more correct diagnosis and certification in Scotland than in England. This view does not, however, explain why the female English is

higher than the female Scotch curve; and it must be assumed therefore that there is some condition more favourable to the causation of cancer in English than in Scotch female life.

The Scottish Widows' Fund curve has the easiest gradient of all, probably pointing to more accurate diagnosis and certification than for the whole country, especially at the earlier periods.

That the apparent increase of cancer is at any rate chiefly due to improved diagnosis is shown by a comparison of the male and female curves respectively. They run practically parallel throughout. If cancer had really increased, its increase would probably have been an approximately equal percentage in the two sexes, and consequently the curves would have widened their distance apart. Even if—assuming that a real increase of cancer had occurred—the increase were unequal in amount in the two sexes, it is highly improbable that the increase would have been of such a distribution as to maintain the parallelism of the male and female curves.

The statistics for Frankfort-on-the-Main enable us to classify cancer in accordance with the part of the body primarily affected. We have, therefore, classified the returns into two groups, according as the cancer is " accessible " or easy of diagnosis, and " inaccessible " or difficult of diagnosis. The results of this classification show that in those parts of the body in which cancer is easily accessible and detected there has been no increase in cancer mortality between 1860 and 1889. It is true that the majority of the deaths from "accessible" cancer are among women—the deaths from " accessible " cancer among men at Frankfort-on-the-Main being too few to be, when considered alone, trustworthy—but we know of no reason for supposing that, while female cancer of "accessible " parts has remained stationary, male and female cancer of the other parts of the body has really increased.

The general conclusions arrived at are that—

1. Males and females suffer equally from cancer in those parts of the body common to man and woman, the greater prevalence of cancer among females being due entirely to cancer of the sexual organs.

2. The apparent increase in cancer is confined to what we have called inaccessible cancer. This is shown (*a*) by the Frankfort figures; (*b*) by the fact that the difference between the rates for males and females respectively is approximately constant, and does not progressively increase with the apparent increase in cancer in each of the sexes; and (*c*) because the apparent increase in cancer among the well-to-do assured lives, who are presumably attended by medical men of more than average skill, is not so great as among the general population.

3. The supposed increase in cancer is only apparent, and is due to

improvement in diagnosis and more careful certification of the causes of death.

IV. " Further Experimental Note on the Correlation of Action of Antagonistic Muscles." By C. S. SHERRINGTON, M.A., M.D. Communicated by Professor M. FOSTER, Sec. R.S. Received April 15, 1893.

(From the Physiological Laboratory of St. Thomas's Hospital, London.)

Appropriate excitation of the afferent nerves from the flexor muscles of the knee joint so alters, as I have shown,[*] the condition of the extensor muscles of that joint that the reaction called the "knee jerk" becomes no longer elicitable. I have endeavoured to examine the quality of the alteration which thus restrains or abolishes the " jerk."

It must be remembered that there is some variance of opinion as to the nature of the jerk itself. In the opinion of some authorities the jerk is of reflex nature (Bowditch, Lombard, Senator, Warren); in the opinion of others it is not truly reflex, but is a direct muscular reaction, intimately dependent, however, on a reflex tonus in the muscle (Tschiriew), or on a spinal influence reflexly exerted, but not necessarily identical with "tonus" nor necessarily measurable by tonicity (Waller).

On the reflex theory of the "jerk," its disappearance or decrease under excitation of the sensory nerve from its antagonistic muscles tallies with phenomena of the mutual interference of spinal activities such as are exemplified perhaps most clearly by those experiments of Goltz, in which, after section of the spinal cord in the thoracic region, the act of micturition could be cut short by strong stimulation of the skin of the tail. On the view that the jerk is not itself reflex, but depends on a reflex tonus, the abeyance of the phenomenon under excitation of the afferent fibres of the hamstring nerve might be owing to decrease thus induced in the tonus of the vasto-crureus muscle, just as on the same view abolition of the jerk by cutting the sensory roots of the crural nerve is due to the impairment thus produced in the tonicity.

As a step toward determining between these two possibilities, I have attempted to discover whether afferent impulses ascending from the hamstring muscles affect to any considerable extent the tonus of the antagonistic quadriceps extensor. Complete abeyance of the "jerk" under excitation of the hamstring nerve cannot, so far as I have seen, be long maintained. After a longer or shorter interval the jerk

[*] 'Roy. Soc. Proc.,' February 1, 1893.

returns. Soon after discontinuance of the stimulation the jerk not only returns, but becomes markedly exaggerated. Though abolition or decrease of the jerk is thus temporary, exaltation conversely produced by severance of the hamstring nerve is permanent, lasting at least for some weeks. On the tonicity hypothesis concerning the jerk, exaltation of the jerk by section of the hamstring nerve should be accompanied by a concurrent increase in the tonus of the vasto-crureus muscle.

It is not easy to judge *slight* differences in tonus of muscles even where of the fellow muscles of the two sides of the body one normal muscle is available as standard for comparison. Yet any decided difference of tonus must, if multiplied into a sufficient time, amount to a not inconsiderable difference in the chemical condition of the muscle. I have therefore sought to test what influence, if any, section of the hamstring nerve exerts on the extensor muscle of the joint as judged of by the development of rigor mortis in that muscle.

In muscles paralysed by section of their nerves, onset of *post-mortem* rigidity is delayed (Kölliker,[*] Brown-Séquard[†]). Bierfreund[‡] has found that after semisection of the spinal cord made a few hours before death the onset of rigor in the limbs on the side of section is considerably later than on the opposite side. Inasmuch as depression of tonus is an alteration in the direction of this paralysis, I hoped that time of onset of rigor mortis might serve to indicate whether, *cæteris paribus*, decrease or increase of tonus had obtained in the muscle for the period immediately preceding death. A few experiments on the influence of nerve section upon speed of onset of rigor mortis gave results accordant with the original by Brown-Séquard and Kölliker. I then turned to the division of, instead of muscular nerves, the anterior roots supplying the muscles. Experiments made on the hind limb of the Cat showed the effect of section of the anterior roots to be a marked delay in the onset of rigor in the muscles supplied by them. The effect was clear, even if the roots were cut only five minutes before killing the animal. I then experimented on division of the posterior roots. The posterior roots of the 6th, 7th, and 8th post-thoracic nerves of one side were severed. In result the hamstring muscles of the corresponding side became rigid later than did those of the opposite side, even in an experiment in which the animal was killed only a quarter of an hour after section of the roots. The effect of section of these sensory roots after previous severance of the cord at the 1st lumbar segment was then examined. The severance of the cord, as was to be expected, deferred considerably the onset of rigor mortis in both of the hind limbs. It appeared also to decidedly

* ' Archiv f. Path. Anatomie,' vol. 10, p. 242, 1856.
† ' Gaz. Méd. de Paris,' No. 42, 1857.
‡ ' Pflüger's Archiv,' vol. 42, p. 195, 1888.

reduce the difference between the time of onset of the rigor in the two hind limbs; but still, in each case, the hamstring muscles on the side corresponding to the section of the posterior roots entered rigidity later than in the fellow limb. The effect of section of the hamstring branch of the sciatic trunk upon the time of onset of rigor in the extensors of the knee, the cord having been previously divided at the 1st lumbar segment, was then proceeded to. Twelve experiments were made, but the results obtained were conflicting. In seven of these experiments rigor commenced in the extensor of the side on which the nerve had not been cut before it did in the extensor of the opposite side. In three rigor commenced indubitably rather earlier on the side of nerve section than on the opposite. In two I could not detect that there was any difference between the two sides in the time of onset of the rigor. A point noted in nine of these experiments (it was not looked for in the remaining three) may be mentioned. When death is induced by hæmorrhage, the cord having a short time previously been severed at the top of the lumbar region, various reflexes can be elicited from the hind limbs and tail for some little time after respiratory spasms and all reflexes have vanished from the body in front of the level of the section. There is in the Cat an ear reflex which generally outlasts others under chloroform or ether administration, and often outlasts them by a considerable time. Indeed, so soon as this reflex has disappeared, the respiratory movements are reduced to a dangerous extent. It consists in a laying back of the pinna of the ear, the pinna being frequently twitched several times: it is elicited by sharply twisting the tip of the pinna. The corneal reflex and still more the knee jerk are both extinguished by chloroform and ether for some time before this ear reflex disappears. This reflex may in the Cat, like Dastre's reflex from the gum of the upper jaw in the Dog, be termed the reflexus ultimus. But when death is induced in the manner above mentioned, the knee jerk outlasts the ear reflex by as long in some experiments as four or five minutes; and in the limb in which the hamstring nerve has been divided the jerk persists longer than in the fellow limb.

Finally, I examined the effect of bandaging one knee in full extension, the other in full flexion, after previously severing the cord at the 1st lumbar segment. The bandages after an interval were removed. The jerk on each side was then found to be good, and rather brisker in the knee that had been flexed than in that which had been extended. Death was induced by hæmorrhage. No difference between the time of disappearance of the jerk on the two sides was detected. In each of five experiments performed the quadriceps extensor and the crural muscles became rigid later on the side that had been flexed than on the other side, but the hamstring

muscles became rigid earlier in the leg that had been flexed than in the other. This is not what might have been expected in view of Wundt's* observation that tension in a muscle hastens rigor in it. It must be remembered that Wundt's statement is not based on muscle *in situ*, intact and connected with the spinal cord. The observed fact harmonises with the existence of an augmentation of tonus of extensors in result of excitation of the afferent nerves from their opponent group.

The experiments so far therefore seem to indicate that the direction of the change induced in the extensor muscles by afferent impulses ascending from the flexors is in the direction of increased tonicity, and to strengthen the supposition that the interference with the "jerk" is located in the spinal mechanism of the "jerk."

The marked influence exerted reflexly by the flexors of the knee upon the extensors of that joint suggested search for instances of analogous correlation elsewhere. The delicately correlated muscles of the eyeball offer an experimental advantage in that even slight alterations of their length can be readily observed by inspection. I therefore exposed the inferior oblique muscle (Cat and Monkey), completely detached it from the globus, and then observed the effect of lightly drawing upon it, so as to stretch it between the end held and the end attached to the bony floor of the orbit. Reflex actions were in this way easily obtained, but were inconstant in character; the eyeballs were generally moved, perhaps most frequently conjugately, toward the side corresponding to the stretched muscle. I watched especially to see whether the globes were turned upward or downward, but those movements were far less frequent than movements apparently purely lateral. Sometimes the reflex obtained did not affect the eyes at all; the movement was often a pricking of the ear, either with movement of the eyes or apart from eye movement. Twice the muscle was detached altogether with careful avoidance of injury to its nerve and blood vessel; held between two ivory-tipped forceps, it was then gently stretched. The results were the same as when its one end retained the natural attachment. In all cases, section of the nerve which enters its posterior border to supply it immediately abolished all reply. This nerve is relatively long and easily isolable. Electrical excitation of it can be performed with facility; a weak tetanising current applied to its central end produced the same variable movements as did the stretching of the muscle itself, but apparently less readily.

The inferior oblique muscle was employed for these observations because it can be freely isolated with little injury and displacement of other structures, and because of the length of the nerve branch supplying it. I should have preferred, otherwise, to use the externus

* 'Die Lehre der Muskelbewegung,' pp. 68—72. Brunswick, 1858.

rectus, because of the simplicity of its antagonistic coupling with the internal rectus. But the external rectus it is not easy without disturbance of other parts to isolate sufficiently to feel certain that tension put upon it remains confined to it alone. I adopted, therefore, for examining the antagonism of the external and internal recti the method of paralysing one and then examining the activities of the other. When examining in the Monkey the movements of the digits obtainable by cortical excitation, if movement, *e.g.*, flexion, of the hallux or pollex is elicited, and the nerve to the flexors of the digit be then severed, renewed cortical excitation at the same spot still produces a movement;* this movement is generally in direction the reverse of that previously obtained; this "*reversal*" is, however, not invariably obtained; occasionally there results a feeble movement in the same direction as the movement previously obtained. In three experiments this movement has been so decided as to lead one to re-examine carefully the site of division of the peripheral nerve in order to assure oneself that it had been really severed completely. The persistence of movement in the same direction as before the flexor nerves are severed must indicate that the stimulation applied to the cortex produces at each repetition an inhibition of the tonus of the muscles antagonistic to the flexors, that is to say, of the extensor muscles. My experience of this inhibition is that it cannot, even when it occurs, be demonstrated many times in succession. The phenomenon of *reversal* is, on the other hand, obtained repeatedly with facility.

A power to inhibit the activity of striated muscle has, therefore, to be included among the attributes of the "motor" cortex of the hemisphere. In the coordination of the movements of the eyeballs it appears to play an important and easily demonstrable part. Ferrier† discovered that excitation of a particular portion of this cortex produces a conjugate movement of both eyeballs in a direction away from the hemisphere in which the stimulation is employed. If the appropriate area of the cortex of the left hemisphere be excited the movement is of both eyeballs to the right. My enquiry regarding the correlation of action of the antagonistic internal and external recti in this movement may be stated as three questions. Is the movement carried out:

a. By contraction of the left internal rectus and the right external rectus, their antagonist muscles undergoing the while simply passive traction;

β. By contraction of the above two muscles combined with slighter contraction (steadying) of their antagonistic muscles (left external rectus and right internal rectus); or

* Sherrington, 'Journ. of Physiol.,' vol. 13, p. 671, &c.
† Ferrier, 'Functions of the Brain,' London, 1876.

γ. By contraction of the left internal and right external rectus associated with inhibition of the tonus of their antagonistic muscles (left external rectus and right internal rectus) ?

. The plan of experiment adopted has been as follows :—The appropriate portion of the left cortex having been ascertained by excitation, and having assured myself that the desired conjugate deviation is regularly obtained, I at once sever the 3rd and 4th cranial nerves of the left side between their origin and the point of their entrance into the cavernous sinus. The position of the left eyeball in rest is, immediately after performance of that section, scarcely perceptibly different from what it has been before, nor is usually the pupil dilated immediately, although it soon becomes so. Excitation of the cortex as before is at once proceeded with. The movement obtained is still conjugate deviation of both eyeballs to the right. The right globus appears to move exactly as before; the left globus, on close examination, although it moves, clearly does not move as it did before. Its movement starts usually just perceptibly later than the movement of the right eyeball; the movement, when started, is somewhat slower than that of the right eye, and it travels only some two-thirds as far; on discontinuing the excitation both eyeballs return together. Sometimes the movement of the left eye, instead of starting later than that of the right, starts simultaneously with it; sometimes it starts distinctly earlier, and this especially when the external squint that soon appears has become well developed. But the movement is never seen quite so rapid or so ample as that of the right eye. On excitation of the corresponding part of the right cortex the movement of the left eye outwards may be very slight, especially if there be marked outward strabismus; in two experiments there frequently occurred on excitation of the right cortex together with the usual movement of the right eye to the left a movement of the left eye to the right and sometimes, apparently, to beyond the primary position. A few times a double movement occurred, the left globus at first moving to the left conjugately with the right, and then suddenly reversing its movement and turning inwards to the right. At each discontinuance of the excitation of the right cortex when the eyeballs have reacted by conjugate movement to the left, the left does not return so quickly as the right; indeed it may be many seconds in returning.

. These experiments succeed uniformly, so far as I have seen, with both *Macacus rhesus* and with *sinicus*, even when very young specimens are used. Ferrier and Munk have shown that conjugate movements of the eyes are obtainable from the cortical surface posterior to the " motor " region, and Schäfer, by demonstrating *inter alia* the great difference in the reaction time for the movements in the two cases, has provided an index for the profound distinction that must be

drawn between them. I find that from this posterior region, as well as from Ferrier's "motor" region, can the tonus of the external rectus be inhibited in the orbit of the same side as the hemisphere stimulated after the 3rd cranial nerve of that side has been cut through.

An extremely interesting observation has recently been made by Mott and Schäfer.* Experimenting together, they have seen that by simultaneous excitation of the frontal cortex of both right and left hemispheres the two eyeballs can be set approximately in the primary position, sometimes with a slight degree of convergence. I have, in two Bonnet Monkeys, repeated this experiment after having previously performed section of both right and left 3rd nerves and both right and left 4th nerves at their origin from the brain. After the section there was considerable double divergent squint, and in one of the experiments the strabismus of the left eye was distinctly the greater in degree. The effect of simultaneous bilateral excitation, approximately balanced, of the frontal cortex was to cause both eyes to be rotated inwards up to, and certainly in some trials beyond, the primary position. The double divergent squint was converted into a slight degree of convergence. Here, where the external recti were the only ocular muscles still connected with the central nervous system, convergence must have been due to simultaneous bilateral inhibition of the tonus of the right and left external recti. It is difficult to find any other interpretation for these results than that the excitation of the cerebral cortex, just as it occasionally inhibits the tonus of the muscles of the thumb or hallux, also possesses the power of inhibiting the tonus of ocular muscles, at least of the external straight muscle. Further, it would seem that this inhibitory activity of the cortex is more constantly elicitable experimentally in the case of the muscles of the eyeball than in the case of those of the hand and foot. That the cerebrum nominally exerts a more or less tonic inhibitory influence over the lower local centres subserving muscular tonicity and local muscular reflex action is a widely accepted doctrine. The above observations accord with and, as I think, extend the data for such a belief.

The above experimental results may, it seems to me, be all of them explained on an hypothesis that the cerebral cortex can inhibit muscular tonus and reduce it even to paralysation limit; but it does not seem necessary to suppose that the cortex, although it can thus inhibit tonus in striped muscle, can also inhibit the active contraction of it. This second assumption seems necessary, however, to explain the following result. If, after section of both right and left 3rd and right and left 4th nerves, the left frontal cortex be excited and both eyes made to deviate to the right, excitation of the right cortex (the

* 'Brain,' vol. 17, p. 165.

experiment has not succeeded unless this stimulation be somewhat strong) will not unfrequently cause the right eye to move inwards and sometimes fully up to the primary position. The active contraction of the external rectus appears to be cut short and even converted into a condition of relaxation more complete than when no cortical excitation at all is being employed.

I have watched with interest in *Macacus* the voluntary movements of the eyes after section of the 3rd and 4th nerves. In the early hours after the section, if for instance these nerves have been cut on the left side only, the gaze is readily directed to the left but not so readily to the right. There arises, of course, considerable external squint of the left eye. Neither when the right is directed toward the right nor when it is converged upon a light or other object just in front of the face is there more than a mere trace of movement of the left eye. Twenty-four or forty-eight hours later, when the right eye is turned to right the left eye does perform the conjugate movement, but imperfectly, and more imperfectly and also more variably than under experimental excitation of the frontal cortex.

Another instance in which antagonistic correlation may be examined is that of the muscles which close and open the palpebral aperture. These are at least potentially antagonistic. I find that in the Monkey excitation of the 3rd nerve in the cranium slightly depresses the lower eyelid at the same time that it freely raises the upper. This slight depression, often very slight indeed, is abolished by section of the branch of the 3rd to the inferior rectus muscle. The 3rd nerve (leaving out of consideration the cervical sympathetic, tho action of which in opening the eye, as regards speed, direction of movement, and other characters, is different from the opening obtainable from the cortex) may be termed the nerve which opens the palpebral aperture. The 7th is the only nerve which closes it by active muscular contraction.

This latter statement applies to the Monkey, not to the Cat, because in the Cat, after section of the 7th nerve at the stylomastoid foramen, I find that, although the upper and lower lids remain permanently rather widely apart, the third eyelid (membrana nictitans) shuts at short intervals with an extremely rapid sweep completely over the exposed part of the globus; and when after section of the 7th the animal sleeps the nictitating membrane is partially extended over the front of the ball. The third eyelid is therefore not innervated like the upper and lower from the 7th. Langley and Anderson[*] record that it is not innervated from the 4th, probably not from the 3rd, but that excitation of the 6th causes " great protrusion of it."

If, after section of the 3rd, the appropriate part of the frontal cortex be excited either on the same side as the nerve cut or on the opposite side I find no movement at all result in the lids on the side

[*] ' Journ. of Physiol.,' vol. 13, p. 461.

corresponding to the nerve section, although the opening of the eye the other side is each time quick and wide. That is to say, there is in this instance no evidence of concomitant activity of the antagonistic muscle, either in the sense of contraction or of relaxation. But on shifting the electrodes to the suitable point of the cortex the eyes of both sides reply to the excitation by a sharp closure just as usual.

The well-known dilatation of the pupil elicitable from this region of the cortex might, in view of the above-described examples of inhibition from the cortex, perhaps, be supposed to be related rather to an inhibition of the constrictor action of the 3rd nerve than to cortical augmentation of the influence kept up *viâ* the cervical sympathetic. The dilatation of the pupil under excitation of the frontal cortex certainly is much later in onset than are the movements of the extrinsic ocular muscles, but that difference might be based on the difference in time of response of the two kinds of muscular fibres involved. This supposition is, however, negatived by the fact that division of the cervical sympathetic cut out dilatation that was being regularly elicited from the cortex, the 3rd nerve being undivided, in two experiments I made to test the point. In these instances, therefore, although an extreme dilatation was obtained with, for the cortex, quite weak currents, the cortex reacted by discharge directed through the sympathetic system.

Addition. April 20, 1893.

I would add to the above the following remarkable passage, which I find in ' The Anatomy and Physiology of the Human Body,' by Charles and John Bell (London, 1826, vol. 3). In describing the action of the muscles of the eye, the author, after adverting to some experiments made by himself on the functions of the 4th cranial nerve, says :—

" We have seen that the effect of dividing the superior oblique was to cause the eye to roll more forcibly upwards; and if we suppose that the influence of the 4th nerve is, on certain occasions, to cause a relaxation of the muscle to which it goes, the eyeball must be then rolled upwards.

" The nerves have been considered so generally as instruments for · stimulating the muscles, without thought of their acting in the opposite capacity, that some additional illustration may be necessary here. Through the nerves is established the connection between the muscles, not only that connection by which muscles combine to one effort, but also that relation between the classes of muscles by which the one relaxes and the other contracts. I appended a weight to a tendon of an extensor muscle, which gently stretched it and drew out the muscle; and I found that the contraction of the opponent

flexor was attended with a descent of the weight, which indicated the relaxation of the extensor. To establish this connection between two classes of muscles whether they be grouped near together, as in the limbs, or scattered widely as the muscles of respiration, there must be particular and appropriate nerves to form this double bond, to cause them to conspire in relaxation as well as to combine in contraction. If such a relationship be established, through the distribution of the nerves, between the muscles of the eyelids and the superior oblique muscles of the eyeball, the one will relax while the other contracts."

My experiments described above show the correctness of Bell's supposition, at least as regards the external straight muscles, and that the phenomenon of inhibition of activity under volition and under appropriate cortical faradisation is not confined among the ocular group of muscles to the recti externi, the observations made seem to prove. For instance, I have divided the 6th cranial nerve of the left side at its origin from the brain, and then examined the behaviour of the eyeballs under suitable experimental conditions. The position immediately assumed by the eyeballs was that of slight convergence even when under anæsthesia (not very deep). Subsequently the internal strabismus increased somewhat, to disappear, or almost disappear, when the gaze was voluntarily turned to the right, but to be greatly increased (*i.e.*, the angle at which the optic axes crossed becoming great) when the gaze was directed to the left. The left eye under volitional movement frequently rotated from the inner canthus outward conjugately with the right, but never passed beyond the primary (median, straight-forward) position. On the gaze being directed from the median primary position toward the right, the left eye rotated conjugately with the right eye usually with an apparently perfect symmetry of motion, but not unfrequently with a movement slower and less ample than the right. On the gaze being directed from the median primary position toward the left, the left eye did not move at all, while the right eye moved of course normally. It was frequently seen that the eyes remained turned to the right, apparently resting in that direction; it was so generally when the animal was sleepy or dozing. When the gaze was directed from that position over to the left, the movement of the left eye was not unfrequently, so far as could be seen, perfectly conjugated and symmetrical with that of the right eye, as far as up to the middle line of the palpebral fissure; there it stopped short, while the right eye went a variable distance further.

On excitation of the appropriate part of the frontal cortex of the right side, both eyes being in the primary position or with a slight degree of convergence at the commencement of the excitation, the right eye swept sharply to the left, and the left either did not move

or merely shifted sluggishly up to the full primary position. On excitation of the frontal area of the cortex of the left hemisphere both eyes were directed to the right, the left eye sometimes moving sluggishly as compared with the right; after cessation of the excitation, the right eye would frequently return at once to approximately the primary median position, while the left eye did not return for a considerable time, and did so in a slow unequal manner; frequently, however, both eyes remained for a long time steadily directly toward the right, and under anæsthesia that seemed to be almost as frequent a position for them to assume as was one approximating to the median primary. When the right frontal cortex was faradised, the eyes resting at the commencement of the excitation in a direction towards the right, the right eye swept over sharply to the left, sometimes with an upward inclination also, more rarely with a downward; movement of the left eye invariably accompanied this movement of the right, and with a corresponding inclination, but the movement of the left eye often started late, and was almost invariably slow and feeble as compared with that of the right, and it was also the more variable. When both eyes had been directed to the right by excitation of the left frontal cortex, and directly after the left side excitation had ceased, the right frontal cortex was excited, both eyes turned toward the left, but the left eye never overshot the primary median plane. When the two eyes had been directed to the right by moderate faradisation of the left frontal cortex, a somewhat strong faradisation of the right frontal cortex, the moderate faradisation of the left side being still continued, caused the left globus, and the right, to be turned toward the left, but the right eye generally started earlier and moved more quickly than the left, and the left, although sometimes brought up to the primary position, was sometimes only slightly turned towards it.* When a moderate excitation of the right frontal cortex had turned the right eye to the left and brought the left to the full median position, moderate faradisation of the left frontal cortex (the excitation of the right side still continuing) turned the left eye to the right so as to produce convergent squint; stronger faradisation of the left cortex successive to and concurrently with moderate faradisation of the right directed both eyes to the right. In two instances moderate excitation of the right cortex alone, near the angle of the precentral sulcus, caused the right eye to be turned to the left, and the left eye to be turned to the right, so that strong convergence resulted.

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* A notable dilatation of both pupils sometimes occurred in this combination experiment. Neither the left nor the right faradisation produced by itself any dilatation of either pupil, but on the second electrodes being applied considerable dilatation of both pupils followed. There was no epilepsy.

Faradisation of the left occipital cortex directed both eyes to the right with a slight downward or slight upward inclination or without any inclination, according to the area excited (Schäfer, Munk). Faradisation of the right occipital cortex, if both eyes were approximately in the primary position, caused the right eye to be slowly and steadily turned to the left, and the left eye to be steadily brought fully up to the medium primary position if it were not already in it; if already fully in the primary position the left eyeball did not move at all. If the left eye moved at all it very generally started its movement distinctly before the right eye and appeared to rotate less slowly than did the right. This difference was best seen if the eyes were previous to the excitation resting in a direction to the right. On then faradising with weak or moderate currents the left occipital cortex the steady slow rotation of the left eye commenced so much before that of the right that sometimes the left had travelled half-way to the primary position before the right eye had well started. Under excitation of the right occipital cortex the movement of the left eye toward the left seemed so clearly stronger, sharper in starting, and even quicker and steadier than under excitation of the right frontal cortex, that I attempted to balance it against the movement to the right produced from the left frontal cortex. On each occasion, when tested, I found that, the two eyes being kept directed to the right by moderate excitation of the left frontal cortex, they could be sent over to the left again by moderate or strong excitation of the right occipital cortex, and in each instance the left or paralysed eye was again the first to start travelling. Three similar experiments gave results quite similar to the above in the points mentioned here.

In two experiments, one on a small *Rhesus* and one on a large *Sinicus*, I have combined with section of the left 6th nerve section of the left 4th nerve at its exit from the brain. The results obtained agreed in many respects with those obtained when the 6th nerve alone was severed. Points of difference were the following:—In the median position of rest the left eye was turned slightly upward as well as inward. On excitation of the right frontal cortex, the eyes starting in the primary position, the right eye was turned to the left side and the left eye was turned upward, more or less amply, rarely not at all; the left eye was never observed to rotate to the left beyond the primary median position. When particular points in the right frontal cortex had been found which directed the eyes not merely to the left but upwards also, on stimulation being applied the left eye was turned upward to a greater degree than was the right. When places were found in the right frontal cortex which directed the right eyeball downward as well as to the left, occasionally the left eye moved a little downwards; more usually it moved distinctly upwards, so that there

was strong divergence of the optic axes in the vertical plane, with the right pupil depressed, the left pupil elevated sometimes considerably. The same held true in the results of excitation of the occipital regions of the cortex, and the left eye generally commenced movement under excitation of the right occipital cortex distinctly earlier than did the right eye, just as when the 6th nerve alone had been divided; so that under excitation of the right occipital cortex there was usually at one phase of the movement instead of any convergence of the optic axes a strong divergence of them.

It was noticeable that after division of the 6th nerve, either alone or in conjunction with the 4th, also in one experiment in conjunction with the 5th, the pupil of the eye on the same side as the nerve section was slightly but distinctly larger than in the opposite eye. This observation led me to stimulate the 6th root in the Monkey, but, although the eye was in result moved outwards, I could not satisfy myself that any constriction of pupil at all occurred, although the pupil was well dilated at the time. In the case of the internal straight muscle, I believed it would be less easy to sever its antagonist's nerve than in the case of the antagonist of the external rectus. I therefore made several earlier experiments, employing section of the external rectus and its nerve inside the orbit. The above description is, however, not based on the results of those earlier experiments in which the orbit was opened, because: (1) It was found then impossible to be quite certain that some remnant of movement or drag in the muscle could not affect the globus sufficiently to simulate the movement that might result from a relaxation of the antagonistic muscle; (2) because when once the orbit has been opened, or its contents dissected, the movements of the globus are deranged sufficiently to beset with doubt any interpretation that can be put upon them; (3) finally, because destruction of the muscles or their nerves in the orbit or cavernous sinus involves destruction also of concomitant sensory fibres from the 5th nerve which may exert a considerable influence on the antagonistic muscle, the subject of observation. I would add, however, that the results obtained in my earlier experiments did, though open to these objections, seem to accord with those I have obtained after sections at the base of the brain; it may be that the harmony between the two sets is superficial rather than real. The experiments from which the results related here are quoted are only those in which neither the orbit nor the cavernous sinus was opened at all.

It appears, therefore, that the activity of the internal straight muscle can be directly inhibited by appropriate excitation of certain parts of the frontal cortex, still better of the occipital cortex, of the hemisphere of the side opposite to the muscle; and the inhibition is very similar to that exerted over the activity of the external

straight muscle by similar regions of the cortex of the *same* side as the muscle. A point of difference—and it is a suggestive one—between the two cases appears to be that under inhibitory relaxation of the *rectus externus* from cortical excitation the globus may rotate beyond the middle line of the palpebral fissure, whereas under cortical relaxation of the *rectus internus* the eyeball may travel up to that middle line, but very rarely, if ever, trespasses beyond it.

It thus seems clear that by experiment abundant support can be obtained for the supposition put forward by Charles Bell.

V. "On the Differential Covariants of Plane Curves, and the Operators employed in their Development." By R. F. GWYTHER, M.A., Fielden Lecturer in Mathematics, Owens College, Manchester. Communicated by Professor HORACE LAMB, F.R.S. Received April 14, 1893.

(Abstract.)

Consider any point (x, y) on a standard plane curve, and write a_1, a_2, a_3, &c., for dy/dx, $d^2y/dx^2 . 2!$, $d^3y/dx^3 . 3!$, &c., the differential coefficients being derived from the equation to the curve. Let $(\xi . \eta)$ be the current coordinates of a point which moves so that $f(\xi, \eta, x, y, a_1, a_2, a_3, \ldots) = 0$, say, on a trajectory of the standard curve. Now let a general homographic transformation affect ξ, η, and x, y alike; the function which replaces $f(\xi, \eta, x, y, a_1, \ldots)$ will generally entirely change its character; if, however, it retains the same form (except that it is affected by a certain factor of which the form is to be found), I define it to be a differential covariant of the standard curve.

It is obvious that among the covariants will be found tangents and polar curves, and the ordinary covariant curves. It is proposed to investigate the subject generally, and to find the relations of the covariant with the contravariant curves.

§ 1.

For the purpose of obtaining the forms of the linear partial differential equations which express the conditions that a function should be a covariant function, an infinitesimal homographic transformation is employed, expressed by the relations

$$\frac{x}{X+B_1Y} = \frac{y}{Y} = \frac{1}{AX+BY+1},$$

where A, B, and B_1 are small, with identical relations for ξ and η.

The conditions found may be stated as follows.

Simple Conditions.

Call the algebraic degree of the equation in ξ η, d, and the algebraic degree of a coefficient in $a_1, a_2, a_3, \ldots, d_x$, which we shall call the degree of the coefficient. In the same coefficient call the sum of the indices of differentiation the weight of the coefficient, and write it w. Write D for the determinant of the homographic substitution,

$$\lambda = (AX + BY + 1)(1 + B_1 Y_1) - (X + B_1 Y)(A + BY_1),$$

$$\mu = AX + BY + 1,$$

$$\nu = A\xi + B\eta + 1.$$

Then (1) the form of the multiplying factor is

$$D^{d+d_0} \cdot \lambda^{-(d+d_0+w)} \cdot \mu^{2w-d_0} \cdot \nu^{-d};$$

(2) ξ, η, x, y, and a_1 only enter in the forms $\xi - x$ and $\eta - y - a_1(\xi - x)$, which will be written $\pi - p$;

(3) The function is homogeneous in $\eta - y$ and the several differential coefficients of y;

(4) The weight of the coefficient of each power of $\xi - x$ is uniform, *i.e.*, each coefficient is isobaric, and this weight, diminished by the index of the power of $\xi - x$, is uniform throughout the function;

Conditions in the form of Linear Partial Differential Equations.

(5) The other equations of condition take the forms

$$(\pi - p)\frac{\partial f}{\partial(\xi - x)} = SS p a_p a_{n-p+1} \frac{\partial f}{\partial a_n} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (A)$$

$$(\xi - x)\left\{ (\xi - x)\frac{\partial f}{\partial(\xi - x)} + (\pi - p)\frac{\partial f}{\partial(\pi - p)} - df \right\} = S(n-2)a_{n-1}\frac{\partial f}{\partial a_n}$$
$$\ldots\ldots\ldots \quad (B),$$

$$(\pi - p)\left\{ (\xi - x)\frac{\partial f}{\partial(\xi - x)} + (\pi - p)\frac{\partial f}{\partial(\pi - p)} - df \right\}$$
$$= SS(p-1)a_p a_{n-p}\frac{\partial f}{\partial a_n} \ldots\ldots\ldots \quad (C),$$

where S denotes summation for all values of n which introduce values of a_n from a_2 upwards, and SS denotes a similar double summation, first with regard to p, which introduces values of a_p between a_2 and a_n, and secondly with regard to n, which introduces values of a_n from a_2 upwards.

These equations will be abbreviated to

$$O_1 f = \Omega_1 f \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{(A)}.$$

$$O_2 f = \Omega_2 f \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{(B)}.$$

$$O_3 f = \Omega_3 f \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{(C)}.$$

§ 2.—*Interpretation of the Conditions* (A), (B), *and* (C).

Write $_r\phi_q\,(\pi-p)^r\,(\xi-x)^q$ as a type of the terms in the covariant. Then (1) $O_1 f = \Omega_1 f$ becomes

$$_r\phi_q = \Omega_{1\ r+1}\phi_{q-1}/q,$$

so that, in any set of terms homogeneous in $(\pi-p)$ and $(\xi-x)$ the coefficients may successively be derived from that of the term in $(\pi-p)^{r+q}$ by means of the operator Ω_1.

And (2) $O_2 f = \Omega_2 f$ becomes

$$_r\phi_q = -\Omega_2\,_r\phi_{q+1}/d-r-q,$$

so that the coefficient of such term homogeneous of degree $r+q$ in $(\pi-p)$ and $(\xi-x)$ may be derived from the coefficients of the set of terms homogeneous of degree $r+q+1$ in $(\pi-p)$ and $(\xi-x)$.

Also (3) $O_3 f = \Omega_3 f$ becomes

$$_r\phi_q = -\Omega_3\,_{r+1}\phi_q/d-r-q.$$

We thus have a second mode of derivation of character similar to the last, showing that the operators are not all independent.

The order of derivation is shown by the chart
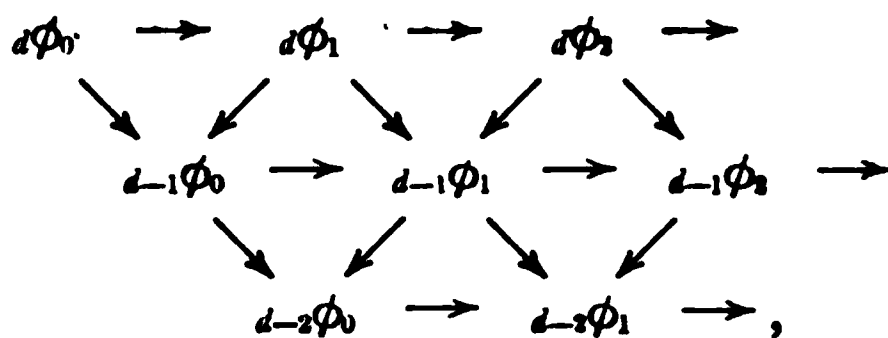
where $\longrightarrow$ denotes deduction by the operator Ω_1,

    $\swarrow$ „   „ Ω_2,

    $\searrow$ „   „ Ω_3

in which it is easy to trace the relations

$$\Omega_2\Omega_1 - \Omega_1\Omega_2 = \Omega_3,$$

$$\Omega_2\Omega_3 - \Omega_3\Omega_2 = 0,$$

$$\Omega_3\Omega_1 - \Omega_1\Omega_3 = 0.$$

From this it is clear that $_d\phi_0$, the coefficient of the highest power of $(\pi - p)$, satisfies the differential equation $\Omega_2 f = 0$, and that the whole expansion can be found if $_d\phi_0$ is known.

On this account I call $_d\phi_0$ the source or matrix of the covariant, and investigate the solutions of $\Omega_2 f = 0$.

It is next proved that

$$\Omega_2 \frac{df}{dx} - \frac{d}{dx} \Omega_2 f = (2w - d_x) f,$$

and hence that, if f is a homogeneous, isobaric function of $a_2, a_3, \ldots$, such that $\Omega_2 f = 0$, and that $2w - d_x = 0$, then df/dx is equally a solution of $\Omega_2 f = 0$.

If we know two solutions of $\Omega_2 f = 0$, solutions of all higher orders can then be educed by the process here implied.

$\Omega_2 = 0$ on expansion becomes

$$a_2 \frac{\partial}{\partial a_3} + 2a_3 \frac{\partial}{\partial a_4} + 3a_4 \frac{\partial}{\partial a_5} + \ldots = 0,$$

and therefore we have two solutions readily, viz.,

$$u_2 = a_2,$$

$$u_4 = a_2 a_4 - a_3^2.$$

The educts successively found by this method are, however, not generally irreducible, and it is shown how to find the irreducible solutions for the successive orders, which are written u_2, u_4, u_5, &c. Any common solution of $\Omega_2 f = 0$ and $\Omega_1 f = 0$ is a differential invariant, and not the matrix of a covariant. For the second and fifth orders, the seventh and all orders higher than the seventh, there is a differential invariant, and for the sixth there is a common solution of $\Omega_2 f = 0$ and $\Omega_3 f = 0$, which I write L_6. It is the matrix of a straight line through x, y, and all matrices may be expressed as functions of u_4, L_6, and differential invariants. The order of the covariant can be inferred from the mode in which u_4 and L_6 enter the matrix.

§ 3.

If the systems of coefficients are subjected to a reciprocal transformation, of which $a\xi - \pi - \gamma = 0$ may be taken as the typical relation, there is this relation between the operators Ω_1 and Ω_2—if u is a solution of either $\Omega_1 f = 0$ or $\Omega_2 f = 0$ in which $a_2, a_3, \ldots$, are the arguments, and if, in consequence of the substitution for these qualities of $A_2, A_3, \ldots$ (similar functions of the correlative system of coordinates), u becomes U, then U is a solution of the other corresponding equation, that is, of $\Omega_2 F = 0$ or $\Omega_1 F = 0$ respectively.

Or, briefly, we may say the result is the interchange of the operators Ω_1 and Ω_2. Regarded as functions of A_2, A_3, &c., the coefficients in a contravariant are developed from a matrix satisfying $\Omega_2 F = 0$, by the process which has been already described. Regarded as a function of a_2, a_3, &c., they are derived from a matrix satisfying $\Omega_1 f = 0$, by a process which differs from that previously described by the interchange of the operators Ω_1 and Ω_2. Any function of the coefficients in a covariant function which would be an invariant for a change of coordinates, but not for a homographic transformation, will be the matrix of a contravariant function. The matrix of the reciprocal is the discriminant of the highest group of homogeneous terms in π and ξ treated as a binary quantic.

§ 4.

To apply these results to the cubic osculating the standard curve at the point (x, y), remove the origin temporarily to that point. The condition of intersecting the standard curve in a number of points coincident with this temporary origin is an invariantal relation, and we may treat the coefficients in the equation as if they contained differential invariants only.

The form of the matrix of the cubic is

$$f u_4{}^2 + (\phi + \phi_1 L_6 + \phi_2 L_6{}^2)\, u_4 + \psi + \psi_1 L_6 + \psi_2 L_6{}^2 + \psi_3 L_6{}^3 + \psi_4 L_6{}^4,$$

with the condition $f + u_6{}^2 \phi_2 + u_6{}^4 \psi_4 = 0,$

where all the functions contain differential invariants only, and, retaining only invariantal coefficients, the expanded equation is

$$\psi \pi^2 - u_2{}^2 u_6 \psi_1 \pi^2 \xi + u_2{}^4 (\phi + u_6{}^2 \psi_2)\, \pi \xi^2 - u_2{}^6 u_6 (\phi_1 + u_6{}^2 \psi_3)\, \xi^2$$

$$- u_2{}^3 \phi \pi^3 + u_2{}^2 u_6 \phi_1 \pi \xi - u_2{}^7 (2f + u_6{}^2 \phi_2)\, \xi^3 + u_2{}^6 f \pi = 0,$$

and writing the differential invariants of the several orders u_2, u_5, U_7, U_8, U_9, we have for the shape of the standard curve near the origin

$$\pi = u_2 \xi^2 + \frac{u_5}{u_2{}^2}\, \xi^5 + \frac{U_7}{u_2{}^4 u_5}\, \xi^7 + \frac{U_8 + 10 u_5{}^4}{u_2{}^6 u_5{}^2}\, \xi^8 + \frac{u_5{}^2 U_9 - 3 U_7{}^2}{u_2{}^6 u_5{}^3}\, \xi^9 + \ldots$$

Substituting in the equation of the cubic, we find

$$\psi_2 = \psi_3 = \psi_4 = 0 \qquad \psi_1 = f, \qquad \psi = -u_6{}^2 \phi_1, \qquad f = -u_6{}^2 \phi_2,$$

and $u_6{}^2 \phi = U_7 f, \qquad U_7 \psi = V_8 f,$

where V_8 stands for $U_8 + 8 u_5{}^4$.

If the cubic is non-singular this determines all the functions in the matrix, and gives the differential equation

$$u_5{}^2 U_7 U_9 - V_8{}^2 - 4U_7{}^3 + u_6{}^4 V_6 = 0.$$

The matrix of the equation to the tangents to the cubic from the origin is

$$(\phi + \phi_1 L_6 + \phi_2 L_6{}^2)^2 - 4f(\psi + \psi_1 L_6),$$

and the condition that the cubic may be nodal or cuspidal is that this matrix, as a function of L_6, may have a linear factor twice or thrice repeated.

In the case of the nodal cubic, the differential equation will be derived from $U_7 \psi = V_8 f$, and, putting $\psi = 2fk$, k is a root of

$$U_7 k^4 - \tfrac{1}{2} u_5{}^4 k^3 + 2U_7{}^2 k^2 - \tfrac{2}{5} u_6{}^4 U_7 k + U_7{}^3 + \tfrac{17}{16} u_5{}^8 = 0,$$

and from this the differential equation is found.

In the case of the cuspidal cubic, put $\psi = 2fk$, $u_5{}^2 \phi = fq$.

Then
$$16 k^3 = 27 u_5{}^4,$$

$$256 q^3 = 27 u_5{}^8,$$

and the differential equation is

$$256 U_7{}^3 - 27 u_5{}^8 = 0.$$

§ 5.

In this section it is attempted to develop a geometrical method, founded on the covariant theory.

The general equation to a covariant line takes the form

$$(u_5{}^2 \phi_3 u_4 + \phi_1 + \phi_2 L_6 - \phi_3 L_6{}^2)\,\pi - u_2{}^2 u_5 \left\{ (\phi_2 - 2\phi_3 L_6) - u_5 \phi_3 a_3 \right\} \xi - u_2{}^3 u_5{}^2 \phi_3$$

$$= 0,$$

and depends upon the invariant ratios $\phi_1 : \phi_2 : \phi_3$.

If we take a second covariant line, the coordinates of the point of intersection take the form

$$\pi = u_2{}^3 A / u_4 A + C + B a_2,$$

$$\xi = u_2 B / u_4 A + C + B a_3,$$

where

$$A = -u_5{}^2 (\phi_2 \phi'_3 - \phi'_2 \phi_3),$$

$$B = u_5 \left\{ (\phi_3 \phi'_1 - \phi'_3 \phi_1) - (\phi_2 \phi'_3 - \phi'_2 \phi_3) L_6 \right\},$$

$$C = (\phi_1 \phi'_2 - \phi'_1 \phi_2) + 2(\phi_3 \phi'_1 - \phi'_3 \phi_1) L_6 - (\phi_2 \phi'_3 - \phi'_2 \phi_3) L_6{}^2.$$

Or, more shortly,

$$A = u_5{}^2 \lambda,$$

$$B = u_5 (\mu - \lambda L_6),$$

$$C = \nu + 2\mu L_6 - \lambda L_6{}^2,$$

and its position depends upon the invariant ratios $\lambda : \mu : \nu$. Treating $(\lambda : \mu : \nu)$ as determining the position of a point, and $(\phi_1 : \phi_2 : \phi_3)$ the position of a line, the condition that $(\lambda : \mu : \nu)$ may lie on $(\phi_1 : \phi_2 : \phi_3)$ is

$$\lambda\phi_1 + \mu\phi_2 + \nu\phi_3 = 0.$$

Hence they form a correlative system of point and line coordinates. I define $(\lambda : \mu : \nu)$ as the invariantal coordinates of a point of homographic persistence, and $(\phi_1 : \phi_2 : \phi_3)$ as the invariantal coordinates of a covariant line.

The condition that $(\lambda : \mu : \nu)$ may lie upon a covariant curve of the nth order will be an invariantal relation, homogeneous of the nth degree in λ, μ, ν, between λ, μ, ν and the invariants in the coefficients of the equation to the curve (we may say the invariantal coordinates) of the curve.

If $f(\lambda : \mu : \nu) = 0$ expresses this relation, it is in this system of co-ordinates the equation to the curve; I call it the intrinsic invariantal equation to the curve.

The coordinates of the tangent to the curve at $(\lambda : \mu : \nu)$ are $\left(\dfrac{\partial f}{\partial\lambda} : \dfrac{\partial f}{\partial\mu} : \dfrac{\partial f}{\partial\nu}\right)$ and $\left(\lambda'\dfrac{\partial}{\partial\lambda} + \mu'\dfrac{\partial}{\partial\mu} + \nu'\dfrac{\partial}{\partial\nu}\right)f = 0$ is the equation to the first polar of $(\lambda' : \mu' : \nu')$ with respect to the curve.

Also writing x for μ/ν and y for λ/ν, the relations between π, ξ and x, y are of the form of a homographic transformation, and therefore any function of $d^2\pi/d\xi^2$, &c., which is a differential invariant, is equal to the identical function of d^2y/dx^2, &c., affected by a factor of known form. Hence, treating $f(\lambda : \mu : \nu) = 0$ or $f(x, y) = 0$ as an ordinary algebraic equation, it will have the same homographic singularities as the original covariant function, while the coefficients are the differential invariants which characterise the curve.

The intrinsic invariant equation to the osculating conic is

$$\lambda\nu + \mu^2 = 0, \qquad \text{or} \qquad y + x^2 = 0,$$

and to the non-singular osculating cubic is

$$u_5{}^4\lambda^2(V_6\lambda + U_7\mu) + (U_7{}^2\lambda - V_6\mu + U_7\nu)(\lambda\nu + \mu^2) = 0.$$

To terminate the abstract, the equation to the polar conic of the origin is

$$U_7{}^2\lambda^2 - V_6\lambda\mu + U_7\mu^2 + 2U_7\lambda\nu = 0,$$

and therefore $U_7{}^2\lambda - V_6\mu + U_7\nu = 0$ is the equation to the common chord of this conic and the osculating conic, and it touches the cubic at $\lambda = 0$, $\mu : \nu = U_7 : V_6$, the tangential of the origin. Also the second tangential of the origin lies on

$$V_6\lambda + U_7\mu = 0,$$

i.e., on the common chord of the cubic and the conic of closest contact.

The third tangential of the origin has the coordinates

$$U_7^2 : -u_5{}^4U_7 : -(U_7 + u_6{}^6),$$

which are independent of V_8. As is known, this is the corresidual of the eight consecutive points at the origin on all the several cubics for which V_8 is arbitrary.

The line $V_8\mu - U_7\nu = 0$, on which the tangential of the origin lies, passes through $(U_7^2 : -U_7V_8 : -V_8^2)$, the sixth point in which the osculating conic meets the cubic again.

The Society adjourned over Ascension Day to Thursday, May 18.

<div align="center">*Presents, May 4, 1893.*</div>

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<p style="text-align:center">*May* 18, 1893.</p>

<p style="text-align:center">The LORD KELVIN, D.C.L., LL.D., President, in the Chair.</p>

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. " On Some Circumstances under which the Normal State of the Knee Jerk is altered."*   By J. S. RISIEN RUSSELL, M.B., M.R.C.P., Assistant Physician to the Metropolitan Hospital.   Communicated by Professor VICTOR HORSLEY, F.R.S.   Received March 3, 1893.

(From the Pathological Laboratory of University College, London.)

CONTENTS.

Introduction.

For the inception of the following work, which it is hoped will add some further facts to our knowledge of the most important of the " deep reflexes," the knee jerk, I am greatly indebted to the very kind suggestions of Dr. Hughlings-Jackson.   To him I owe my best thanks for proposing the question of the influence of the asphyxial

state on the knee phenomenon, as a subject likely to be fruitful. The adoption of his early advice led to the investigation of many other points of noteworthy interest.   I am also much obliged to Professor Horsley for suggestions, especially with regard to control experiments and operative procedures.

Since Brown-Séquard* described the condition expressed by him under the term spinal epilepsy, the so-called deep or tendon reflexes have attracted considerable attention, and have been the object of numerous investigations.

Charcot and Vulpian† were the first to accurately describe the phenomenon known as ankle clonus.

Bouchard‡ next observed the arm reflexes, and Erb§ and Westphal simultaneously directed their attention to the contraction of the quadriceps extensor which follows percussion of its tendon.   Erb looked on the phenomenon as a reflex, while Westphal considered it the result of direct excitation of the muscle percussed or pulled at its extremity.

Joffroy endeavoured to prove that cutaneous excitation often evoked the phenomenon, and this view was not refuted until it was shown that it could be evoked in animals by percussion of the denuded tendon.

Westphal pinched, pricked, and irritated the skin in various ways without producing the knee jerk; a fold of skin lifted away from the tendon and subjected to blows with a hammer was attended with a like negative result.   On the other hand, when the skin lying over the ligamentum patellæ was frozen by means of Richardson's process the contraction which followed a blow on the tendon was in no way lessened; nor was it where cutaneous anæsthesia existed in non-tabetic cases.

It soon became evident to observers that the knee jerk is a normal phenomenon, while ankle clonus is only met with in association with abnormal states; but there was some discrepancy of opinion as to whether the knee jerk is always present in healthy subjects. Berger|| noted its absence in 1·567 per cent. of normal individuals; while in over 200 instances Eulenberger¶ never failed to elicit it in the newly born at various ages, and Gowers** states that it is probably never absent in health.

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* ' Journ. de la Physiol.,' vol. 1, p. 475, 1858.
† ' Soc. Méd. des Hôp.,' May, 1866; ' Union Méd.,' p. 464, 1866.
‡ ' Arch. de Méd.,' vol. 2, p. 290, 1866.
§ ' Arch. f. Psych. et Nerven.,' vol. 5, p. 792; *ibid.*, vol. 5, p. 803, 1874; ' Comptes Rendus et Mém. de la Soc. de Biol.,' Series VI, vol. 2, p. 61, 1875.
|| ' Centralbl. f. Nervenheilkunde,' 1879.
¶ ' Correspondenz-Blatt f. Schweizer. Aerzte,' Nos. 1 and 2, 1879.
** ' A Manual of Diseases of the Nervous System,' 2nd Ed., vol. 1, p. 431.

Westphal* connected for the first time the absence of the phenomenon with sclerosis of the posterior region of the lumbar cord, and Petitclerc† showed that it is also absent when the anterior roots of the spinal nerves are affected.   It was about this time that Buzzard‡ called attention to the fact that in conditions where the knee jerk is absent the response of the vastus internus muscle to direct percussion may be actually more brisk than in health.

Exaltation of the tendon reflexes was recognised by numerous observers as existing where muscular spasm, contracture, and "epileptoid trepidation" are present, and that these conditions are associated with sclerosis of the pyramidal tracts of the spinal cord.

In the investigation of afferent nerves in tendons and such as would form the first part of a reflex loop, Tschiriew§ is generally supposed to be the first observer who discovered nerve fibres, without myeline, which terminate in the aponeuroses in the neighbourhood of tendons, which he looked on as the organs of transmission of the muscular sense.   These centripetal nerves had, however, previously been noticed, but imperfectly described, by Sachs.   And Golgi‖ and his pupil Cattaneo¶ fully described the nerve endings in tendons; and, although Golgi's completed work did not appear until 1880, his first publication on the subject was three years prior to this.

Nothnagel** found that when clonus existed it could be arrested instantly by pressure on the anterior crural or sciatic nerve, and that pressure on the former nerve stopped the movements in the territory of the sciatic as well as in its own, and that pressure on the nerves of one side puts an end to the phenomenon on the opposite side. Lewinski†† further showed that in cases of contracture with exalted reflexes increase of the tendon of the tension of the contracted muscle, however brought about, would evoke clonus, as would excitation of the nerve supplying the muscle with a moderate induced current; while cutaneous stimulation by pinching the skin, or by squeezing the toes or fingers, would arrest it, as would excitation of the nerve supplying the muscle with a strong induced current.   If the tendon was first rendered lax, none of the methods which formerly evoked the clonus would do so.

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* ' Arch. f. Psych.,' vol. 8, p. 514.

† " Des Réflexes tendineux," ' Thèses de Paris,' 1880.

‡ ' Lancet,' July, 1878.

§ ' Arch. d. Physiol.,' Series II, vol. 6, p. 89, 1879.

‖ ' Rendiconti del Reale Istituto Lombardo,' fasc. IX, p. 445; ' Gaz. Lomb.,' vol. 7, s. V, p. 23; ' Atti d. Soc. Ital. di Scienz. Nat. a Milano,' vol. 21, p. 464; and ' Memorie della R. Accad. d. Scienz. d. Torino,' vol. 32, 1880, 28 Stu., 2 Tafeln.

¶ ' Arch. Ital. de Biol.,' vol. 10, fasc. III, p. 337

** ' Arch. f. Psych.,' vol. 6, p. 332, 1876.

†† *Ibid.,* vol. 7, p. 327, 1877.

Burckhardt,[*] in attempting to prove that the phenomenon is a reflex, measured the time which elapsed between the percussion of the tendon and the resulting muscular contraction. His investigations led him to the conclusion that it is a reflex which is produced in the spinal ganglia and not in the cord. Following him, numerous investigators endeavoured to determine the time which elapses between the stroke and the contraction.

Brissaud,[†] with Franck, measured the time very exactly in healthy persons, and also in those the subjects of disease. These observers found that 0·05 second elapses in healthy subjects, but that it varies from time to time in the same individual, and is modified by conditions which alter the excito-motor properties of the spinal cord. When the lateral columns of the cord are sclerosed the time is diminished; it is the same on the two sides in a healthy subject, but is less on that side when one lateral column is more sclerosed than its fellow.

Déjerine's[‡] observations and Ter-Meulen's[§] researches furnished very similar results.

Gowers,[||] who registered the movement of the limb in his observations, concluded that 0·09 to 0·015 second is the time which elapses between the stroke on the tendon and the resulting movement of the limb, which time is sufficient to allow a reflex to occur. Percussion of the tibialis anticus gave a response in 0·03 to 0·04 second, which is not sufficient for a cord reflex. He therefore concluded that ankle clonus is a phenomenon of direct excitation, while the knee jerk is to be looked on as a reflex.

Waller[¶] found that the time which elapsed between percussion of the tendo Achillis and contraction of the gastrocnemius was 0·3 to 0·4 second, and in the case of the quadriceps extensor and its tendon 0·3 to 0·4 second. While admitting the necessity of the integrity of the spinal cord for its production, he did not admit its reflex nature, as the time was too short for a reflex to occur. He looked on the phenomena as merely peripheral reactions, and only tests of spinal conditions in the sense that other peripheral reactions of muscles (electrical or mechanical) are tests of these conditions.

Since then new facts with regard to the knee jerk have been recorded by several observers.

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[*] " Ueber Sehnen-Reflexe," ' Festsch. V. Haller,' Bern, 1877.

[†] ' Recherches Anatomo-path. et Physiol. sur la Contracturo perm. des Hémipl.,' Paris, 1880.

[‡] ' Comptes Rendus,' May, 1878.

[§] ' Ueber Reflexprikkenbaarhied en Pcssreflexen,' Amsterdam, 1879.

[||] ' Lancet,' Part I, p. 156, 1879.

[¶] ' Brain,' Part X, 1880.

Hughlings-Jackson* has recorded an instance of exaggeration of the knee jerk on one side after convulsions which affected chiefly the leg on that side. This observer has also published a case in which the knee jerks, absent in a case in which the posterior columns of the cord were sclerosed, returned when the lateral pyramidal tracts became sclerosed consequent on a cerebral hæmorrhage. Dr. Jackson has also observed that the knee jerks are absent in conditions attended by hyper-venosity of the blood in man.

Beevor† found that after generalised convulsions in man the knee jerks were absent or exaggerated according as the convulsions were very severe or less so, and the former condition he ascribed to exhaustion of the lumbar centres consequent on their severe discharge of energy.

Jendrassik‡ has furnished us with a valuable method of eliciting the knee jerk when it appears to be absent in healthy persons. It consists in making the person clench the hands or perform some other violent movement at the moment when the patella tendon is struck, when the resulting jerk is found to be increased. He concluded that the knee jerk is a true reflex caused by mechanical irritation of the nerves in the tendon; that passive tension of the muscle is necessary for its production; that voluntary innervation of the crural nerve lessens or prevents it; and that innervation of the sciatic favours it.

Weir Mitchell, and Lewis,§ from a large number of investigations in man, supplemented by a few in dogs and rabbits, concluded that the knee jerk varies in health, may be exhausted by too much use, and may increase from frequent excitation. All volitional acts may increase it; and weak innervation of the crural nerve increases it, as does any form of innervation of the sciatic, while strong prohibits it. Continual violent muscular actions, as of both arms and hands, eventually enfeeble the knee jerk. Passive tension is not essential for its production, moderate tension mechanically favours it, and extreme tension destroys it even in spastic cases. An act of will directed to a part functionally inert, or to amputated parts, reinforces the knee jerk. Strong or weak stimulation of one sciatic in etherised animals intensifies the knee jerk of the other leg; and pressure on the sciatic in man sufficient to cause pain and numbness diminishes the knee jerk on that side. One knee jerk does not reinforce the other. Touch has no effect on it, but all abrupt impressions such as pain, heat, and cold anywhere on the skin increase it, as does a

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* 'Med. Times and Gaz.,' February, 1881; 'Brit. Med. Journ.,' July, 1891; February and March, 1892.

† 'Brain,' Part XVII, 1883.

‡ 'Deutsches Arch. f. klin. Med.,' vol. 33, p. 177, 1883.

§ 'The Medical News of Philadelphia,' February 13th and 20th, 1886.

violent optical impression.   Nitrite of amyl has no effect on the jerk, but ether abolishes it in dogs, but has a smaller effect in rabbits. Faradic currents strong enough to produce contraction of muscles increase the knee jerk, and stimulation of the dry skin has a like effect.   Short voltaic currents not strong enough to cause muscles to contract are attended by the same result, as are voltaic currents applied to the head, especially the temporal regions and especially when the negative pole is applied.   Making is more effectual than breaking the current, and the effects soon wear away.   Long ascending galvanic currents to the spine cause marked increase, while descending cause far less, and moderate currents do not reinforce the knee jerk.   These results led these observers to conclude that the knee jerk is a direct muscular act, but that it cannot exist without that spinal contribution known as tone, which is capable of increase by a variety of causes.   They consider that it cannot be a reflex, as the latter are inhibited by violent sensory stimulations, which they have shown increase the knee jerk.   An exactly opposite view is that taken by Lombard,* who observed that the flexors, *i.e.*, the hamstrings, sometimes contracted when the ligamentum patellæ was struck.   He concluded that the flexors were caused to contract by reflex excitation, and that the whole phenomenon was of the same nature.

Bowditch and Warren† found that when a voluntary action was employed to produce exaltation of the knee jerk the reinforcement wholly depended on the interval between starting of the action and the incidence of the blow to elicit the jerk.   When this was prolonged, instead of being exalted, the knee jerk became much reduced; the interval at which the effect changes from positive to negative varied from 0·22 to 0·6 second.   The effect of sudden auditory or visual stimulus was usually positive.   Tactile stimulation of the conjunctiva and of the nasal mucous membrane also showed the same effect.

Bastian‡ has recently brought forward striking evidence to prove that in man total transverse lesion of the spinal cord above the lumbar enlargement abolishes, instead of augmenting, the knee jerks, as is commonly supposed; and Bowlby§ has supported this view by the results of his observations in fracture of the spine with crushing of the cord.

Buzzard‖ has met with absence of the knee jerk in man when there was extravasation of blood into the cranial cavity.

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* ' Journ. of Physiol.,' vol. 10, p. 122; ' American Journal of Psychology,' vol. 1, p. 50, 1887.

† ' Journ. of Physiol.,' vol. 11, p. 25, 1890.

‡ ' Med. Chir. Trans.,' vol. 73, 1890 ; ' Lancet,' March, 1890.

§ ' Med. Chir. Trans.,' vol. 73, 1890; ' Lancet,' May, 1890.

‖ ' Med. Press and Circ.,' March, 1890.

History of Previous Experimental Work.

Experimental work directly bearing on the subject commenced with Schultz and Fürbringer,[*] who noted absence of the knee jerk after section of the crural nerve or lumbar roots in rabbits. A like result followed the administration of curara. They also showed that by striking the tendon on one side, under certain circumstances, not only that knee jerk could be elicited, but also that on the opposite side. The conclusions come to were that it is not the result of direct irritation of the muscle or its tendon, but a reflex act, the centre for which is situated in the lower part of the cord, and that there can be no question of its being in any sense a cutaneous reflex.

Tschiriew[†] excited the divided crural nerve electrically in order to give the muscle tonicity, but never succeeded in obtaining the knee jerk after it had disappeared on section of the nerve.

Burckhardt,[‡] experimenting on rabbits, concluded that the duration of the phenomenon is less than the time necessary for a cutaneous reflex, that it subsists after section of the posterior roots of the spinal nerves and after destruction of the cord, but that it is abolished by section of the crural nerve; hence his belief that it is a reflex produced in the spinal ganglia and not in the cord.

Tschiriew,[§] with the knowledge that the crural nerve in the rabbit is formed by the 5th and 6th lumbar roots, divided the cord at the level of the 3rd lumbar, and found that the knee jerk became exaggerated, while when the section was made between the 5th and 6th roots the knee jerk disappeared. On making successive sections from the sacral region upwards he found that the knee jerk remained intact until the level of the 6th lumbar pair was reached. He divided the posterior roots of the 6th lumbar pair, taking great care not to injure the neighbouring parts, and found that such section abolished the knee jerk. Destruction of the segment between the 5th and 6th lumbar pairs resulted in abolition of the knee jerk also.

Prevost,[||] in his first series of experiments, divided the cord in the dorso-lumbar region, whereupon the reflexes below this point became exaggerated. Section of the crural nerve abolished the knee jerk, as did section of the posterior root of the 6th lumbar in the rabbit, while it was augmented when the sciatic alone was divided. These experiments, therefore, confirmed the results of previous observers, especially those of Tschiriew. The next series of experiments were

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[*] ' Centralbl. f. d. Med. Wiss.,' p. 929, 1875.
[†] ' Arch. f. Psych. und Nerv. Krank.,' vol. 8, p. 694.
[‡] *Loc. cit.*
[§] ' Arch. de Physiol.,' Series II, vol. 6, p. 293, 1879.
[||] ' Rev. Méd. de la Suisse Romande,' February, 1881.

entirely original on the part of Prevost, and deserve careful notice. He found that in the cat deep chloroform narcosis, and the administration of curara, each individually caused abolition of the knee jerk, while profound ether narcosis, almost up to the point of cessation of respiration, failed to do so. In the rabbit, on the other hand, profound ether narcosis abolished the knee jerk. Intravenous injection of 1 gramme of chloral in 10 grammes of water, injected in small quantities into a cat until the animal was profoundly insensible, failed to have any effect on the knee jerk. 3 grammes of water containing 0·075 gramme of the hydrochlorate of morphia, injected into a rabbit in the same way, caused exaggeration of the knee jerk, the animal being in a state of torpor with diminished respiration. The third series of experiments were also original, and consisted in compressing the abdominal aorta, and thus producing anæmia of the cord, and observing the effect of this on the knee jerk. Such compression, whether employed through the integument or applied directly to the abdominal aorta, in the rabbit caused in a few seconds exaggeration, and then abolition of the knee jerk in about 45 seconds, as a rule, and it remained absent for from 15 or 20 seconds to one or several minutes, after the compression was left off, before returning. The length of time that it remained absent was found to be directly proportional to the length of time that the compression on the aorta had been kept up; the longer the compression, the slower was the knee jerk in returning. The results obtained from the various methods of experimentation led Prevost to believe that the knee jerk has a decidedly central origin, and is of the nature of a reflex, also that it is the excitation of the tendon, and not of the skin, which elicits it. He further believed that the contraction of the opposite quadriceps when the tendon of one side is struck pointed to the existence of a crossed reflex.

Waller and Prevost[*] soon after performed some experiments in conjunction, which proved this last conjecture to be erroneous. They divided the sciatic, anterior, crural, and posterior roots on one side, and yet did not abolish the crossed movement. Percussion of the tendon on the injured side caused no contraction on that side, but evoked a contraction on the opposite side at least as vigorous as before the injury. From this they concluded that there was no physiological transmission, and therefore no "crossed reflex," but that the result was due to physical diffusion of vibrations.

Senator[†] found that the hemisection of the spinal cord affected the knee jerk on the same side only; that section or greater disturbance of the posterior columns of the lumbar cord, or of the posterior horns had no effect on the jerk; and that dividing the lateral

[*] ' Rev. Méd. de la Suisse Romande,' June, 1881.
[†] ' Arch. f. Anat. u. Physiol.,' Physiol. Abth., 1880, Heft 2.

column at the level of the 5th to the 6th lumbar vertebræ augmented the jerk on that side.

The latest experimental work on the subject is that by Sherrington,* who found that section of those branches of the anterior crural which supplied the vastus internus and adjoining part of the crureus abolished the knee jerk, while when these branches were left intact, and all the other branches of the nerve divided, the knee jerk remained active. He also noted that section of the 6th roots in rabbits did not always abolish the knee jerk, but occasionally section of the 5th root alone sufficed to bring about this result. In the cat, this observer found that when all the anterior and posterior nerve roots of the lumbo-sacral region, with the exception of the 6th, were divided, the knee jerk became brisk; but that section of the 6th root instantly abolished the jerk. When only half the filaments which composed the posterior root of the 6th root were alone divided the knee jerk was at once extinguished. Section of the anterior root of the 6th root greatly diminished, but did not absolutely abolish, the jerk; when, however, the ventral root of the fifth was subsequently divided in the same animal the knee jerk was abolished. In *Macacus rhesus* division of the 5th root never failed to abolish the knee jerk, while section of all other adjacent roots only tended to render it more brisk. Section of the whole, or even half, of the posterior root of the 5th abolished the knee jerk; but when the efferent root of the 5th was divided a remnant of the jerk persisted, as long as the efferent root of the 4th lumbar was left intact. Bisection of the cord in the lumbo-sacral region of *Macacus rhesus* produced no alteration in the knee jerk. Transverse section of the cord at the level of the 9th thoracic root caused the knee jerk to disappear ten minutes later, and it did not commence to return until three weeks after. A similar lesion of the cord at the level of the 1st lumbar root, with partial section at the 3rd lumbar, caused abolition of the jerk, and it did not return during the six months that the animal was under observation. When the knee jerk was abolished by the inhalation of chloroform it did not return so rapidly when the cord had been previously divided transversely as when it was left intact.

In a later paper† Sherrington has greatly added to our knowledge of the nerve arc on which depends the efficiency of the jerk. After showing how very readily the afferent fibres in the 5th lumbar posterior root (*Rhesus*), and on which the existence of the knee jerk depends, can be impaired, he proceeded to expand the observation of Tschiriew concerning the hamstring nerve, and he then found that excitation of this branch only of the sciatic trunk inhibited the jerk (thus excluding for the most part skin paths), and further that

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* ' Journ. of Physiol.,' vol. 13, No. 6, 1892.
† ' Roy. Soc. Proc.,' February 9, 1893.

stretching (moderately and gently) a hamstring muscle would produce the same effect, thus proving that afferent impulses from the hamstrings normally inhibit the jerk.

The General Plan of Research.

In the present research various circumstances under which the knee jerk is altered are dealt with, and may, for convenience of description, be best arranged in two large groups.

A. Those circumstances under which the alteration in the state of the knee jerk is brought about by some influence locally exerted on the lumbar centres.

B. Those circumstances under which the alteration is due to some remote cause. In the first of these groups are included :—

I. The effects of asphyxia.

II. The effects of the inhalation of certain gases (nitrogen, nitrous oxide, and oxygen).

III. The action of anæsthetics (ether and chloroform).

IV. The effect of anæmia of the spinal centres as brought about by compression of the abdominal aorta, or by general loss of blood.

V. The action of intravenous injections of absinthe and strychnia respectively.

VI. The result of bisection of the spinal cord made vertically at the level of the centres in which the knee jerk appears to be represented.

The second group includes :—

I. The effect of removal of a cerebral hemisphere.

II. The immediate and late effects of extirpation of different parts of the cerebellum.

And as control experiments in this connexion.

1. The effects of extirpation of the labyrinth.
2. The effects of intracranial section of the 8th nerve.
3. The effects by chemical excitation of the 8th nerve.

In every instance those circumstances under which the knee jerk is exaggerated, and those under which it is abolished, are considered ; but, inasmuch as both conditions may be brought about during different stages of the same influence, both of these states of the knee jerk are considered together.

Operative Procedure.

The anæsthetic employed to render the animal unconscious was invariably ether, except in those instances in which the effect of the action of chloroform on the lumbar centres was under consideration, when that anæsthetic was alone made use of throughout the experiment.

In every instance tracheotomy was performed, and narcosis kept up by allowing the animal to inhale the anæsthetic through the tracheotomy tube. When the effects of the inhalation of certain gases were under observation the tracheotomy tube was connected with a ⊤-shaped glass tube by means of a short piece of india-rubber tubing; and india-rubber tubing was connected with the other two limbs of the ⊤-tube, by which means it was easy to so arrange that the animal should inhale the particular gas from its reservoir, and exhale into the air of the room, the one tube being connected with the reservoir, and the other left free.

In those instances in which the heart beats were recorded the right carotid was exposed, opened, and a cannula inserted into it, which in turn was in connexion with a mercurial manometer. When a graphic record of the respiratory excursions was obtained it was made by means of a bertambour, whose elastic membranes were attached to a strip of Leslie's strapping which encircled the thorax. From this tambour an india-rubber tube led to a Marey recording tambour by which the respiratory excursions were registered on the cylinder.

In studying the effects of anæmia of the cord, the abdominal aorta was compressed, either by means of the thumb through the integuments, or directly after it had been exposed by dissection. General blood letting was effected by exposing and dividing one or both carotids.

Intravenous injections were made by injection with a hypodermic syringe into either a jugular or femoral vein, the minute opening made by the needle being closed by a small clip.

Bisection of the spinal cord was effected by exposing the cord at the level of the roots concerned with the knee jerk, inasmuch as division of it abolishes the phenomenon, and by means of a delicate thin-bladed knife, making a vertical incision along the middle line of the cord to the extent of two inches, with the root at its midpoint, and extending through to the anterior surface of the cord. In removing a cerebral hemisphere, a small disc of bone was first removed from one side of the cranium by means of a half-inch trephine, and then the opening was enlarged by bone forceps until the whole hemisphere was exposed. The dura mater was then dissected off, and the hemisphere removed *en masse*, or taken away piecemeal by means of a sharp scoop.

In the case of the cerebellum, a skin incision was made along the middle line down the back of the neck, and at its upper extremity a horizontal incision was carried across the vertex. The tissues were then scraped and cut away, until the occipital bone and arch of the atlas with the intervening occipito-atloid membrane were exposed; care being taken to work from the middle line outwards, as by this means hæmorrhage was found to be much less troublesome. When the superior vermis, or part of it, was to be removed, the bone covering this region was removed by means of bone forceps, and then the portion of the vermis desired excised by means of the knife. But when one or other lateral lobe was to be removed the occipital bone on that side was trephined by a half-inch trephine, and the opening increased by means of bone forceps. The dura was next dissected off, and then by means of a sharp scoop the lobe was scraped out. A syringe containing hot water (about 100° F.) was used to aid in the removal of the broken down cerebellar tissue, and in the arrest of hæmorrhage. The plan adopted in extirpating the labyrinth consisted in making a curved incision, commencing above, passing behind, and ending below the ear. The flap of skin with the pinna was then turned forward, all structures divided and scraped from the bone, so as to expose the meatus and the bone for a short distance around. The upper and posterior part of the meatus was then enlarged by means of a gouge, and by degrees the middle ear and then the labyrinth were cut into and cleared out.

When the 8th nerve was being exposed the skin incision varied little from that described above; but the bulla was more freely denuded of its soft coverings. The bulla was opened by means of a gouge, after which its inner wall was similarly cut through, until the 8th nerve was exposed. It was found that no appreciable length of the nerve could be exposed without lifting up the lateral lobe of the cerebellum, but that the nerve could be divided without interference with this structure. This plan of operation was sometimes followed in extirpating the labyrinth.

In both these methods of operating, hæmorrhage was profuse and troublesome, but was checked by the use of aseptic wax.

In those instances in which the animal was to be afterwards allowed to live the operation was conducted on strict antiseptic principles, the edges of the wound brought together by means of horse-hair sutures, and the wound dressed antiseptically.

In any instance where the operation was so severe as to lead one to suppose that the animal would be conscious of pain after the effect of the ether had passed off, a subcutaneous injection of ½ or 1 grain of morphia was, as usual, given at the close of the operation, and repeated if necessary.

A. Circumstances under which the Alteration in the State of the Knee Jerk is brought about by some Local Action on the Lumbar Centres.

I. *The Effects of Asphyxia.*

Asphyxia was produced either by clamping a short piece of india-rubber tubing fixed to the free extremity of the tracheotomy tube, or by inserting an india-rubber plug into the tracheotomy tube directly. Before inducing asphyxia the knee jerks were carefully and repeatedly tested in order to be certain that the depth of narcosis was not such as to cause any material alteration in the state of their condition. When it was satisfactorily determined that they were as nearly as possible normal, cardiac and respiratory tracings were taken for some seconds before the tracheotomy tube was plugged. The immediate effects on the respiratory and cardiac movements do not call for description; that they were those usually met with under similar conditions is shown in fig. 1.

1. *The Effects of Asphyxia on the Lumbar Centres with the Spinal Cord Intact.*—The first effect which the altered conditions of the blood had on the knee jerk was to cause it to become exaggerated; this quickly became more and more marked, until clonus at the knee was produced by a single tap on the patella tendon; and when the exaggeration was at its maximum a tap on one tendon not only produced clonus on that side, but also evoked the knee jerk on the opposite side, even to the extent of a few clonic jerks. After this stage was reached the knee jerk became less and less marked, until it disappeared completely, and did not return up to the time of the death of the animal, when this was allowed to take place. But if, instead of allowing the animal to die, the obstruction to the entrance of air into the lungs was removed, and the animal recovered either with or without the aid of artificial respiration, the knee jerk reappeared. The time which elapsed before its reappearance depended on the length of time that the asphyxial state had been kept up; the shorter the duration of this state the quicker did the knee jerk return, and *vice versâ*. On its return the knee jerk did not at once present its normal characters; at first feeble, it quickly passed into a state of exaggeration, sometimes to the extent that a single tap on the patella tendon evoked a few clonic jerks; after a variable period this exaltation subsided, and it returned to its normal condition. The stages in the asphyxial state to which these different phenomena are related are indicated in fig. 1, which shows the respiratory and heart tracings obtained from a dog. About 2 minutes after the trachea was clamped the stage of asphyxial convulsions was reached, and after the blood pressure had risen the knee jerk

became exaggerated. This exaggeration amounted to clonus in about 30 seconds later; but about 1½ minutes later, and after the stage of asphyxial convulsions, the knee jerk was abolished, while the blood pressure was still high, though showing signs of commencing to fall. Artificial respiration was commenced about a minute after this (see fig. 2), and 1½ minutes afterwards the knee jerks returned, became exaggerated in about 50 seconds, and returned to their normal state about 2½ minutes later.

The exact times at which the various phenomena occurred varied in different animals of the same class, and more widely in animals of different classes. Fig. 3 shows the approximate times at which they occurred in one of the rabbits used. In this particular instance the knee jerk became exaggerated about 50 seconds after the tracheotomy tube was plugged, and it was abolished about 70 seconds later. The plug was removed from the tracheotomy tube about 10 seconds after this, and in about 10 seconds the knee jerk returned; in about 12 seconds it became exaggerated, and returned to its normal condition after 60 seconds. In fig. 4, besides the blood and respiratory curves, a tracing of the asphyxial convulsions is shown, as obtained from the extensor muscles of the forearm of the dog, in order to show that the knee jerks were still exaggerated for some time after the convulsions had ceased.

It will thus be seen that exaltation of the knee jerk is a phenomenon of the so-called first and second stages of asphyxia, while its abolition is related to the third stage of that condition. Figs. 5 and 6 are intended to show what relationship exists between the alteration in excitability of the spinal centres and that of the cortical cells. Although the cortex exhibits some diminution in its excitability before the knee jerk is lost, yet its excitability is not completely annulled until some time after all attempts to elicit a knee jerk have failed. The preliminary exaggeration of the knee jerk commences to show itself before there is any sign of diminution of the cortical excitability.

2. *The Effects of Asphyxia on the Lumbar Centres after Total Transverse Section of the Spinal Cord in the Mid-dorsal Region.*—By this means any influence of the cerebrum and cerebellum on the phenomena was entirely excluded. It was found that, with the exception of commencing the experiment with a knee jerk more active than when the cord was not previously divided, the phenomena evoked by the asphyxial state were in every way identical to those observed before the cerebrum and cerebellum were excluded from taking any possible part in, or exerting any influence upon, their production.

II. *The Effects of the Inhalation of Certain Gases.*

1. *Nitrogen.*—As has been already noted, a ⊤-tube was made use of in these experiments; one limb of the ⊤ being connected with the tracheotomy tube, one by means of india-rubber tubing, with the gas reservoir, and one being left free with a small piece of india-rubber tubing fitted on to its extremity. The tube leading from the gas reservoir was grasped between the fingers and thumb of one hand, while the piece of tube at the extremity of the free limb of the ⊤ was grasped between the fingers and thumb of the other hand. During inspiration the free end was closed, while the tube from the bag of nitrogen was left free, and during expiration the tube from the nitrogen was closed, and that opening into the air of the room was opened. In this way nitrogen gas was alone inspired, while expiration took place into the air of the room.

Except that it took a longer time to obtain the results, the effects were identical with those met with in asphyxia.[*] The loss of knee jerk was preceded by the stage of exaltation, and when the animal was allowed to recover the absence of knee jerk was followed by a stage of slighter exaltation before its return to the normal state, such as existed before the administration of the nitrogen.

2. *Nitrous Oxide.*—The plan of procedure was the same in these experiments as in the last, and the results differed from them in no way. Loss of the knee jerk was always preceded by a stage of exaltation, and when the animal was allowed to recover, the normal state of the knee jerk was only reached after passing through a period during which it was exalted.

3. *Oxygen.*—Here again the plan of procedure was the same as in the experiments with nitrogen; but the results were widely different. Indeed, they were totally different from any that had been previously obtained, for in no case was the knee jerk abolished, no matter how long the so-called apnœic state was kept up. Increased activity of the knee jerk, gradually becoming more so until clonus developed, was the only effect of inhalation of this gas. When its administration was discontinued the knee jerk gradually returned to the condition in which it was before the inhalation of oxygen was commenced.

III. *The Action of Anœsthetic Agents.*

1. *Ether.*—In profound narcosis induced by the inhalation of ether the knee jerk is abolished; but this stage is preceded by one of increased activity, and followed by a similar stage of exaltation when the animal is recovering from the effects of the anæsthetic, and before *the knee* jerk returns to its normal conditions. The length of time

* *Cf.* Martin, 'Journ. of Physiol.'

that the knee jerk remains absent depends entirely on the depth of narcosis, and the length of time this is continued, these bearing a direct ratio to each other. The exaggeration of the knee jerk which has been alluded to is never so pronounced as in simple asphyxia or the condition induced by nitrogen or carbonic oxide; but it is nevertheless perfectly distinct.

2. *Chloroform.*—The results obtained were identical with those obtained with ether, with two important exceptions, however; thus, the knee jerk is lost very much more rapidly under its influence than under the influence of ether, and after the administration of the narcotic is discontinued a longer period elapses before the knee jerk shows any sign of returning than after a similar depth and duration of ether narcosis.

These effects with ether and chloroform were obtained both when the spinal cord was intact and when it had been previously divided transversely in the mid-dorsal region.

IV. *The effect of Anæmia of the Spinal Centres as brought about by Compression of the Abdominal Aorta or by General Blood-letting.*

1. *Compression of the Abdominal Aorta.*—This was effected by means of the thumb, either through the structures composing the abdominal wall in the rabbit, or after the artery had been exposed by dissection (rabbit and dog). The resistance offered by the bony spinal column makes it exceedingly easy to entirely stop the flow of blood in the aorta beyond the point at which compression is made by the thumb. As Prevost[*] found, after the compression had been kept up a short time the knee jerk disappeared, and the time that it continued absent after the compression had been taken off varied directly as the time that the compression had been maintained; the longer the compression, the longer did the knee jerk afterwards take to return. Before the knee jerk was abolished, and after it was abolished and before it returned to its normal state, it passed through stages of exaltation, as has been already observed to be the case in other conditions attended with loss of the knee jerk.

2. *General Blood-letting.*—The carotid artery was opened on one or other side, and the animal allowed to gradually bleed to death. As more and more blood was lost so the knee jerk became more and more active at first; but after a time it gradually became less so, until it eventually disappeared entirely, and did not reappear up to the time of the death of the animals.

[*] *Loc. cit.*

V. *The Action of Intravenous Injections of Absinthe and Strychnia.*

1. *Absinthe.*—The action of this drug on the lumbar centres was tried, both when the spinal cord was intact and after transverse section of it in the mid-dorsal region. As has been explained, a jugular or femoral vein was exposed, and varying doses of the essential oil of absinthe were injected into it, according to the class of animal under observation and the particular effect which was aimed at. Doses short of those necessary to evoke generalised convulsion were attended by increase of the knee jerk, whether the cord was intact or divided in the dorsal region. Doses sufficiently large to evoke powerful generalised convulsions were also followed by increase of the knee jerk after the convulsions ceased.*

When the spinal cord had been previously divided transversely in the dorsal region, and the lower extremities thus excluded from the otherwise general convulsions, the increase of the knee jerk, after large doses of absinthe, was still observed. The increase never amounted to clonus or tonus, but was nevertheless well marked. This effect of absinthe on the lumbar centres was strikingly demonstrated in the following experiment. The spinal cord of a dog was divided transversely in the mid-dorsal region when the animal was so profoundly under the influence of ether that the knee jerk could not be obtained. The animal was then kept lightly under the influence of the anaesthetic, but, in spite of this, the knee jerk had not returned fifteen minutes after the cord had been divided. Three minims of the essential oil of absinthe were then injected into the femoral vein of one side, and the knee jerk of the opposite side kept under observation. Well-marked convulsions occurred in a few seconds, limited to those parts above the spinal cord lesion, while the parts below the lesion remained inactive. At the time that the convulsions above the cord lesion were at their height there was doubtful evidence of return of the knee jerk. As there was absolutely no sign of the presence of the knee jerk ten minutes after the first injection of absinthe was given, 2 minims more of the essential oil were injected into the same vein. As before, convulsions occurred in those regions whose nerve supply was derived from the central nervous system above the lesion in the spinal cord, while those regions which derived their supply from below this point remained passive. A few seconds after this further administration of the drug there was undoubted return of the knee jerk, which persisted for a minute or two and then disappeared. Ten minutes after the second dose of absinthe was given a third dose of 2 minims was injected into the same vein as on the two former occasions with exactly similar results with respect to the convulsions. The knee jerk was obtained a few seconds after the

* Beevor, *loc. cit.*

absinthe had been injected, and did not again disappear in the course of half an hour, during which time it was under careful observation.

2. *Strychnia.*—The effect of this drug was only tested in the dog. Two minims of a 1 per cent. solution of strychnia were injected into the femoral vein, after the spinal cord had been previously divided transversely in the mid-dorsal region. Powerful convulsions quickly followed in all parts whose nerve supply was derived from above the cord lesion, while those whose nerve supply was from below this point remained free from convulsion. The knee jerk was increased at first slightly, but soon more so, and on injecting 1 minim more of the same solution of strychnia into the femoral vein as before, a single tap on the patella tendon was sufficient to evoke tonus in the quadriceps extensor.

When strychnia was administered without the spinal cord being previously divided, general convulsions of course resulted, and these seriously interfered with the observations on the knee jerk, as even when the animal was not in a comparatively quiescent state a tap on the quadriceps tendon usually sufficed to evoke convulsions. The plan of first dividing the spinal cord transversly in the dorsal region was therefore that usually adopted in observing the effects of strychnia on the knee jerk, and in all cases there could be no question as to the increase of the jerk under the influence of this drug.

VI. *The Results of Section of the Spinal Cord.*

1. Transverse Section.
2. Vertical Section.

1. *Transverse Section of the Spinal Cord.*—It has long been recognised that in the lower animals complete transverse section of the spinal cord above the level of the lumbar enlargement is followed by increased activity of the knee jerk. My own experiments in the dog show that the immediate effect of such a lesion depends on the depth of narcosis. The cord was always divided at or about the mid-dorsal region, and in no instance did the knee jerk disappear after such section of the cord when there had been no doubt as to its presence before the lesion was inflicted, while usually there was no difficulty in being certain of its increased activity under such circumstances. The depth of ether narcosis, however, plays a most important part in this connexion, for if the cord were divided when narcosis was so profound that the knee jerk was almost abolished, the section of the cord hastened the result; and when the cord was divided when the depth of narcosis was such that the knee jerk was abolished, the latter remained absent for a much longer time than is usual after

suspension of the administration of the anæsthetic when the spinal cord has not been divided.

2. *The Effect of Bisection of the Spinal Cord vertically at the level of the centres on whose activity the knee jerk depends.*—These experiments were undertaken to ascertain whether or no the centres to which the knee jerks are related are separate and independent in their action on the two sides, or whether the centre on one side is in any way related to its fellow of the opposite side, and depending on its co-operation for the proper performance of its functions. Since these experiments were performed Dr. Sherrington has published the results of similar experiments carried out by himself in connexion with the lumbar cord of *Macacus rhesus*. My results entirely agree with his, and showed that dividing the cord into two lateral halves, the incision commencing at the level of the 1st lumbar roots and extending to the level of the 1st sacral pair, had no appreciable effect, either in increasing or diminishing the knee jerks.

B. Circumstances under which the Alteration in the State of the Knee Jerk is due to some Remote Cause.

I. *The Effect of the Removal of a Cerebral Hemisphere.*

Either hemisphere was selected, and during the process of exposing it great care was taken to prevent the animal from losing more blood than was absolutely unavoidable. When everything was ready for the removal of the hemisphere the knee jerks were carefully tested, so as to be certain that nothing had occurred to render them unequal during the preparatory operation. The hemisphere was then excised after the dura mater was dissected off it. The effect was immediate and striking, the knee jerk on the opposite side of the body becoming distinctly more active than that on the same side as that on which the cerebral hemisphere was removed, this inequality being due to an increased activity of the knee jerk on the opposite side, and not to a diminished activity of that on the same side.

II. *The Immediate and Late Effects of Extirpation of Portions of the Cerebellum.*

1. *The Immediate Effect of Extirpation of Portions of the Cerebellum.* —*a.* The removal of one lateral lobe was followed by great exaggeration of the knee jerk on the same side, while that on the opposite side was considerably diminished. So great was the exaltation of the knee jerk on the same side as that on which the cerebellar lobe was *excised,* that often a single tap on the patellar tendon sufficed to produce tonic extension of the limb.

When after extirpation of one lateral lobe of the cerebellum the opposite one was also removed, the knee jerk on the side corresponding to that on which the lobe was more recently removed became increased, while there was some diminution of the knee jerk on the side on which the lobe was first removed, the two knee jerks being as nearly as possible equal, and, while both knee jerks were exaggerated, they were not as much so as was the one when only one cerebellar lobe was removed.

The depression of the knee jerk on the opposite side under these circumstances was a phenomenon less marked than was the exaltation of that on the same side as that from which the cerebellar lobe had been removed. In order to test this point further, one cerebral hemisphere was first removed, with the result that the knee jerk on the opposite side became more active than it was before.

A curious and interesting ether effect was observed in connexion with this difference of the knee jerks on the two sides when one lateral lobe of the cerebellum was removed. While the above results obtained during ordinary moderate ether narcosis, the result was exactly the reverse when anæsthesia was very profound, for then the knee jerk which was formerly exalted became depressed, even abolished, while that which was formerly lessened became markedly exaggerated.

The lateral lobe of the cerebellum on the same side as that on which the cerebral hemisphere was removed was next extirpated, when the active knee jerk on the opposite side became less so.

On account of the risk of complication by injury of other parts, no attempt was made to do more than remove the whole of the *posterior* part of the vermis of one lateral half of this posterior portion. When only the half of this posterior portion was removed, there was slight increase of the knee jerk on the same side, but this increase was in no way comparable to the great exaggeration which followed removal of one lateral lobe. As far as could be determined, the knee jerk on the opposite side was in no way altered by this lesion. When the whole of the posterior portion of the vermis was removed both knee jerks became slightly increased.

2. *The Late Effects of Extirpation of Portions of the Cerebellum.—*
On the day after the operation of removal of one lateral lobe of the cerebellum the knee jerk on the same side was still exaggerated, though less so; but although the knee jerks on the two sides were still unequal, that on the opposite side was no longer feeble, but was slightly exaggerated. In the course of the second and third days it became difficult to be certain that any inequality existed on the two sides, both knee jerks being very much exaggerated, though neither of them as much so as was the one on the side of the lesion immediately after the operation. A month or six weeks after the

operation most of the animals showed a slight inequality of the knee jerks, so that, although both were exaggerated, that on the side corresponding to that from which the cerebellar lobe had been removed was slightly the more so; but in some animals the inequality was not evident. When half of the posterior part of the vermis was removed, it was usually difficult next day to say which knee jerk was the more active; but, as in the case of the lateral lobe, after a few weeks a difference on the two sides could be once more detected in most of the animals. At this time the knee jerk on the opposite side was usually as nearly as possible normal, while that on the same side as the cerebellar lesion was more active than normal. In like manner the increase of knee jerk produced by removal of the whole of the posterior portion of the vermis persisted.

If, after a one-sided lesion, either of the vermis or lateral lobes, the animal was placed under the influence of ether, the inequality of the knee jerks became more pronounced; and if the anæsthetic was pushed to the point at which abolition of the knee jerks results, that on the opposite side usually disappeared sooner than did that on the side corresponding.

There seemed, therefore, little reason to doubt that the effects obtained in connexion with the knee jerk when portions of the cerebellum were removed were directly the result of the cerebellar lesion. However, Professor Victor Horsley kindly drew my attention to the fact that the proximity of the auditory nerve and labyrinth made it necessary to institute control experiments, with a view to excluding the possibility of the effects being wholly or partly due to interference with one or other, or both, of these structures. He suggested that the labyrinth should be extirpated on one side in some animals, while in some others the 8th cranial nerve on one side could be subjected to chemical excitation. I accordingly performed the following control experiments.

III. *Extirpation of the Labyrinth on one side.   (Control.)*

No inequality was produced in the knee jerks by this at the time of the operation, nor was any alteration detected two weeks after the operation, except in one instance, in which, two weeks after the operation, the knee jerk on the same side as the ear lesion seemed slightly more active than its fellow of the opposite side. As there was suppuration in the deep wound in this case, it was thought that the result probably depended on entrance of pus into the region of the lateral lobe of the cerebellum on the same side, but no such condition was macroscopically obvious at the autopsy, so that this case *is difficult* of explanation.

IV. *Intracranial Division of the 8th Nerve on one Side.   (Control.)*

No inequality in the knee jerks was produced, either at the time of the operation, during the few hours immediately following the operation, or at any time during the week, after the section of the 8th nerve.

V. *Chemical Irritation of the 8th Nerve on one Side.   (Control.)*

It was found impossible to expose a sufficient length of the nerve to apply an irritant to it without lifting up the lateral lobe of the cerebellum. As any interference with the cerebellum would have vitiated the experiment, it was decided that, instead of applying the irritant directly on the nerve, it should be placed in the labyrinth instead. Accordingly the labyrinth was opened, and a small portion of the 8th nerve exposed as it entered the labyrinth. Crystals of chloride of sodium were next carefully packed around the stump of the nerve and kept in position by means of a plug of aseptic wool introduced into the labyrinth. The knee jerks were carefully tested immediately after the operation, and frequently during the same day, but no alteration could be detected in them, and they remained normal and equal on the two sides during the week that they were kept under observation.

SUMMARY AND CONCLUSIONS.

I. *The Effects of Asphyxia.*

The results obtained in asphyxia appear to be due in great measure to cutting off the supply of oxygen to the lumbar centres. The preliminary exaggeration of the knee jerk might have been due to the taking off of cortical control from the lumbar centres, letting them go, so to speak, as suggested by Hughlings-Jackson,[*] for, as numerous experimenters have shown, asphyxia diminishes, and in an extreme degree annuls, the excitability of the motor cortex. That this hypothesis cannot be entertained is proved by the following facts. At the time when the knee jerk first becomes exaggerated the excitability of the motor cortex is little, if at all, diminished, and is not annulled until a considerable time after the knee jerk is abolished. Further, and this appears to me more conclusive evidence, the same exaggeration of the knee jerk was met with when asphyxia was induced in animals whose spinal cords were divided above the lumbar enlargement, in which case any cerebral influence was entirely put out of court. As far as the preliminary exaltation of the knee jerk is con-

* 'Brit. Med. Journ.,' February, 1882.

cerned, therefore, we must look to the spinal centres alone for explanation of the phenomenon. The rise of blood pressure which occurs in the first stage of asphyxia might be held accountable for the exaltation of the knee jerk; but that this is not the true explanation seems evident, owing to exaltation being met with in anæmia of the cord, and in the marked fall of blood pressure in general blood letting. That it is due to an irritable condition of the lumbar cells induced by depriving them of their normal supply of ·oxygen is rendered probable from the following facts, as well as those just mentioned.

What part, if any, the presence of excess of carbonic acid in the lumbar centres plays in the production of the phenomenon, it is difficult to estimate. That the absence of oxygen is alone capable of producing it is shown by the effects of anæmia of the cord, and by the fact that when nitrogen gas is inhaled the same result is obtained; for in this latter instance most of the oxygen in the body is used up, and the nitrogen takes its place, but there is not necessarily any excess of carbonic acid, since there is no obstruction of expired air, merely the entrance of nitrogen instead of oxygen being provided for. One thing is, however, evident, viz., that in all these conditions the effect is not so rapid as in asphyxia; therefore it is not improbable that the presence of excess of carbonic acid hastens the results.

The abolition of the knee jerk might be due to exhaustion of the lumbar centres by the asphyxial convulsions, for Beevor[*] has shown that after severe epileptic convulsions in man the knee jerk may be abolished, and this has been attributed to exhaustion of the lumbar centres, consequent on their great discharge of energy. That this is not the explanation of the phenomenon in asphyxia is proved by the fact that it occurs when the spinal cord has been divided transversely in the dorsal region prior to the occurrence of asphyxial convulsions, and where, therefore, the lumbar cells are not discharged during the asphyxial spasms, and consequently cannot be exhausted in this way. Indeed it appears not improbable that the effects in man which have been attributed to exhaustion of the lumbar centres may after all be due to the asphyxiated condition of the blood, which occurs in a severe epileptic seizure, consequent on the arrest of respiration due to the tonic spasm of the muscles of respiration.

That the failure of the knee jerk is consequent on the fall of blood pressure which takes place in the third stage of asphyxia seems improbable, as the knee jerk is lost just when the blood pressure commences to fall, often, indeed, before there is any distinct evidence of the decline of blood pressure. Then also in general loss of blood the blood pressure falls very considerably lower than the height at which it stands when the knee jerk is lost in asphyxia without any *sign of loss of* knee jerk, which does not take place till much later.

[*] Loc. cit.

I trust, therefore, we are justified in concluding that the failure of the spinal centres, to which the knee jerk is related, to perform their functions is due in large measure to their being starved of oxygen; for this same starvation occurs in asphyxia when nitrogen or nitrous oxide is inhaled, and when anæmia of the lumbar cord is produced; in all these conditions abolition of the knee jerk being the ultimate result observed.

With regard to the part played by carbonic acid in helping to bring about this effect, what has already been said with reference to the preliminary exaltation of the knee jerk is wholly applicable; therefore nothing further need be said on this subject.

The behaviour of the knee jerk when it reappears after being absent for a time, taken in connexion with its behaviour when first deprived of its normal supply of oxygen, and before it was abolished, suggests the probability that not merely the presence of oxygen is necessary for the lumbar cells to functionate normally, but a certain definite quantity of that gas. And this normal balance appears to be upset either by an insufficient quantity of the gas, or by too much of it; for when pure oxygen is inhaled the same exaltation is observed.

II. *The Effects of the Inhalation of certain Gases.*

The results obtained with nitrogen and nitrous oxide, coinciding as they do with those obtained in asphyxia, lend strength to the hypothesis which has been advanced in explanation of the phenomena related to asphyxia; for when either of these gases is inhaled, to the entire exclusion of all atmospheric air, the blood is principally deprived of its oxygen, and so also the lumbar centres. Here, again, it is therefore probable that the absence of oxygen is to be looked on as the prime factor which brings about the alterations in the knee jerk, which have been already detailed, the exact form of alteration apparently depending on the amount of oxygen left in the blood. When the amount of oxygen in the blood is small, or only moderate, the lumbar centres become more excitable, while when it is absent, or only present in very minute traces, inaction of these centres results.

III. *The Effects of Ether and Chloroform.*

That the results obtained with these drugs are due to their direct action on the lumbar centres is shown by the fact that the results are almost identical, whether these centres have or have not been cut off from their connexion with higher centres by complete transverse section of the cord in the dorsal region. It is a little curious that the lumbar centres should show the same increased excitability when

these poisons have acted on them for a certain time, just as when they have been deprived of a certain amount of their supply of oxygen. That they should fail to act after they have been subjected to the influence of such powerful poisons is not surprising. It may, of course, be urged that the results obtained with these drugs are not the result of their specific action on the lumbar centres, but of the want of oxygen. While it cannot be denied that this may play some part in the production of the phenomena, that the direct action of the poisons is mainly responsible there can be little doubt, for during the respiration of these vapours, in the manner employed in their administration to animals, the entrance of atmospheric air is not excluded, so that, while possibly the interference with the supply of oxygen to the lumbar centres might account for the preliminary exaltation of the knee jerk, it can never be nearly so great as to bring about the abolition of the knee jerk, which, as has been shown, takes a considerable time to disappear, even after the fresh supply of oxygen to the blood has been completely cut off. Further, and this is more convincing, the effects of chloroform and ether respectively differ in two important respects, since (1) the effects on the knee jerk are brought about very much more rapidly when chloroform is inhaled than when ether is administered, and (2) the effect produced by chloroform continues longer after its administration is discontinued than does the same effect when produced by ether. Nothing but certain inherent properties of these drugs can be reasonably believed to be the causes of these differences, for, if venosity of blood was alone or mainly responsible for the results, the effects produced by ether should be more rapid and powerful than those produced by chloroform, since greater venosity is observable in the case of the former than the latter drug. It seems not unreasonable to assert, therefore, that the results obtained with ether and chloroform are mainly due to the direct action of these drugs on the lumbar centres.

IV. *The Effect of Anæmia of the Spinal Centres.*

Deprivation of the lumbar cord of its blood supply, either by direct pressure on the abdominal aorta or by general blood letting, is synonymous with depriving it of its normal supply of oxygen. But another factor has to be taken into consideration in this relation, for both of these methods of experimentation are attended with notable lowering of the blood pressure in the lumbar cord. That the shock occasioned by such lowering of blood pressure, whether resulting from the more or less sudden effect of clamping the aorta, or the more gradual lowering in general loss of blood, is capable of causing *abolition of the knee jerk is not improbable*; but that this is the true *explanation of the occurrence of the phenomena is made improbable,*

when it is remembered that exaltation of the knee jerk precedes its disappearance, for in no known method of producing shock of the spinal cord is there an increased excitability of its centres before their failure to act. It seems, therefore, more feasible to attribute the whole of the result, as far as exaltation of the knee jerk is concerned, to the absence of oxygen.

V. *The Action of Intravenous Injections of Absinthe and Strychnia.*

On the supposition that the former of these drugs acts mainly on the cerebral cortex, and possibly in some degree on the bulbar centres, but little, if at all, on the lower spinal centres, this drug was given in doses sufficiently large to produce powerful generalised convulsions, in order to test whether or no there is any evidence of exhaustion of the lumbar centres after such discharges, when the paths from these centres to the cerebrum, and from the cerebrum to them, are left intact. It was found that, in spite of the most powerful and oft-repeated generalised convulsions, there was increased action rather than depression of the lumbar centres, as evinced by increase of the knee jerk. That this was not a fair test of the condition of the lumbar centres, as comparable to their condition when generalised convulsions result from some condition which evokes discharge from the higher centres alone, is proved by the results obtained with absinthe, when the lumbar cord had been severed from its connexion with the higher centres by complete transverse section of the cord in the dorsal region. When this was done it was found that, though no convulsions in those parts enervated from the spinal cord below the level of the transverse lesion, yet there was abundant evidence of a considerable direct action on the spinal centres, as proved by the re-establishment of the knee jerk when its absence probably depended on shock to the lumbar centres, and of its increased action when it was comparatively normal before the administration of the drug. That strychnia should cause increase of the knee jerk, whether the spinal cord was intact or divided transversely in the dorsal region, was only what was expected from the well-known powerful action of the drug on the spinal cord. It is thus clear that absinthe, like strychnia, has a direct action on the lumbar centres, but that this action is very much less pronounced in the case of the former than in that of the latter drug.

VI. *The Results of Section of the Spinal Cord.*

That complete transverse section of the spinal cord, sufficiently high up not to interfere with the lumbar centres either by direct mutilation, shock, or myelitic softening consequent on the lesion, does not

abolish the lesion in the lower animals, but, on the contrary, causes distinct increased activity of it, is a well-established fact, which did not require the further support which the results of these experiments give to it. The chief interest in the present experiments, however, is centred in the varying effect obtained according to the depth of ether narcosis ; and they demonstrate very clearly how greatly shock to the spinal cord may be increased by the depth of narcosis. That is, a lesion of such severity and in such a situation as to cause very little or no shock to the lumbar cord under moderate ether narcosis may be made to produce considerable shock if the depth of narcosis be greater at the outset.

The results of bisection of the spinal cord in the lumbo-sacral region prove, in confirmation of Sherrington's observations, conclusively that the centres on whose action the knee jerk depends are distinct in the two lateral halves of the cord, and are not dependent on each other, either for control or for reinforcement.

The Effects of Removal of a Cerebral Hemisphere.—The increase of the knee jerk on the opposite side, which followed the removal of the cerebral hemisphere of one side, is a result in keeping with the best established views of the relation of the lower spinal centres to the higher cortical ones; for by removing the hemisphere the control which the cortical centres are regarded generally as exercising over the spinal centres of the opposite side chiefly was removed.

The Immediate and Late Effects of Extirpation of portions of the Cerebellum.—As has been seen, the knee jerk on the same side became exaggerated both when one lateral lobe of the cerebellum or when one half of the posterior part of the vermis was removed, and that this increase of the knee jerk persisted for weeks, *i.e.*, after all phenomena which could be ascribed to " irritation " had passed off. It seems justifiable therefore to attribute this to a " paralytic " rather than an " irritative " lesion. This being the case, two hypotheses are open to us in explanation of the phenomenon : either we must suppose that the one half of the cerebellum exercises an energising influence on the opposite half of the cerebrum, in virtue of which it causes the opposite cerebral centres to hold in greater check the spinal centres of the side of the spinal cord corresponding to the side of the cerebellar lesion; or that the one half of the cerebellum exercises a controlling influence on the spinal centres on the same side of the spinal cord. If the first of these hypotheses is accepted, then the increase of the knee jerk is due to the diminished power of control of the cerebral centres on those in the spinal cord in consequence of the loss of the energising influence of the cerebellum on the cerebrum. If, on the other hand, the second of these hypotheses be accepted, the increase of the knee jerk is due directly to the taking off of cerebellar control from the spinal centres. That the first of these hypotheses

does not wholly meet the case is, I think, clear on two grounds. The increase of the opposite knee jerk which is consequent on the removal of the cerebral hemisphere of one side is not nearly so great as that brought about by the removal of the lateral lobe of the cerebellum of the same side. When one cerebral hemisphere has been first removed, and the opposite lateral lobe of the cerebellum is removed, the knee jerk on the opposite side from the cerebral and on the same side as the cerebellar lesion is further increased after the second operation. It thus seems clear that the cerebellum exercises an independent action on the spinal centres from that exercised by the cerebrum, and that this action is of the nature of a control over the spinal centres chiefly of the same side. That this is the chief explanation of the phenomenon there appears little doubt; but it does not entirely negative the possibility of the first hypothesis being wholly or in part true.

The fact that the diminution of the knee jerk, on the side opposite to that on which the lateral lobe of the cerebellum was removed, was always of such short duration, points to that phenomenon as being an "irritative" one, possibly one of inhibition.

The curious ether effect which was observed in this connexion is easy of explanation as far as the knee jerk of the opposite side is concerned, when it is considered alone; for, if it be admitted that the inhibitory influence of the cerebellar lesion is sufficient explanation for the occurrence of diminution of the knee jerk, it is easy to understand how the first effect of the ether would be to remove this inhibitory influence, and by its direct action on the spinal centres to cause exaggeration of the knee jerk. The effect of the ether on the knee jerk of the same side as the cerebellar lesion is also easy of explanation, when considered by itself, for the direct effect of the ether on the spinal centres, when present in sufficient quantity, would be to cause diminution, and finally abolition, of the knee jerk. When both phenomena, *i.e.*, those of the opposite sides, are considered together, the explanation becomes more difficult; for, in order that the explanations suggested when the phenomena were separately considered should hold good now that they are considered in conjunction, one of two things must be admitted: either some influence on the spinal centres of the side corresponding to that of the cerebellar lesion has rendered them less capable of resisting the direct action of the ether; or some influence acting on the spinal centres of the opposite side has rendered them more resistant to the action of that drug.

These questions must for the present remain unanswered.

That interference with the 8th nerve or labyrinth plays no part in the phenomena which follow extirpation of parts of the cerebellum is abundantly proved by the control experiments which were performed in order to decide this question.

DESCRIPTION OF FIGURES.

...d to show the stages in the asphyxial state at which the knee jerks
...aggerated and are abolished.

...re from carotid artery.
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...stage when the knee jerk returns, after it has been abolished, and
...mes exaggerated before returning to its normal condition.

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... the asphyxial convulsions.

...ve from carotid artery.
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DESCRIPTION OF FIGURES.

FIG. 1 is intended to show the stages in the asphyxial state at which the knee jerks become exaggerated and are abolished.

 a = Curve from carotid artery.
 b = Respiratory curve.
 c = Time curve.

FIG. 2 shows the stage when the knee jerk returns, after it has been abolished, and that it becomes exaggerated before returning to its normal condition.

 a = Curve from carotid artery.
 b = Respiratory curve.
 c = Time curve.

FIG. 3 indicates the stages in the asphyxial state at which the alterations in the condition of the knee jerk occur in the rabbit.

 a = Curve from carotid artery.
 b = Respiratory curve.

FIG. 4 shows the stages in the asphyxial state at which the knee jerk becomes exaggerated and is abolished, and how these phenomena are related, in point of time, to the asphyxial convulsions.

 a = Curve from carotid artery.
 b = Respiratory curve.
 c = Time curve.
 d = Curve obtained from the extensor muscles of the wrist during the asphyxial convulsions.

FIG. 5 shows that a response can be obtained on excitation of the motor cortex after the knee jerk has been abolished.

 a = Curve obtained from carotid artery.
 b = Respiratory curve.
 . c = Curve obtained from extensor muscles of the wrist on a single excitation of the cortex cerebri.

FIG. 6 shows that the motor cortex remains excitable after the stage at which the knee jerk is abolished.

 a = Curve from carotid artery.
 b = Curve obtained from the extensor muscles of the wrist on repeated excitations of the cortex cerebri.
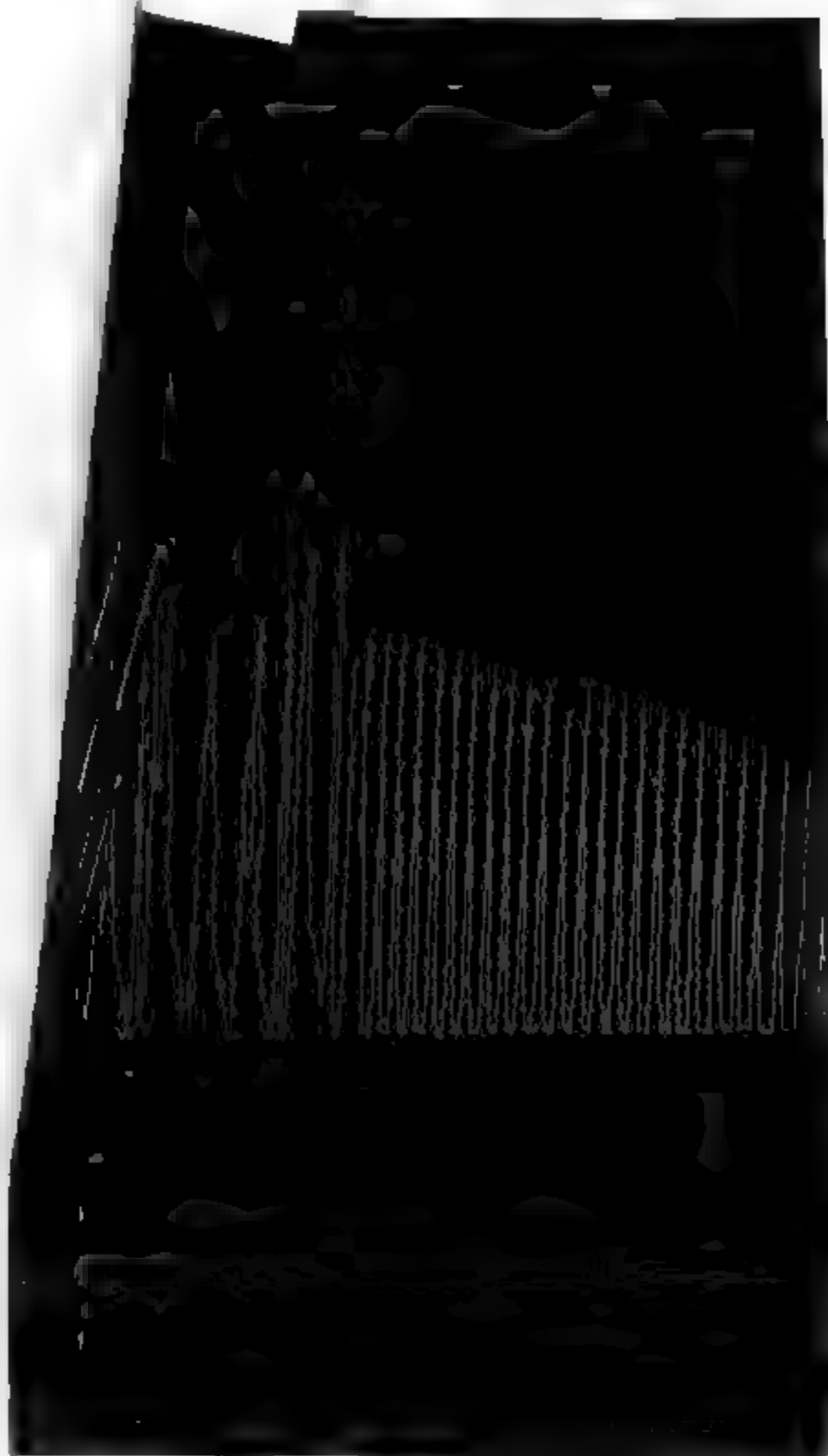
II. "An Experimental Investigation of the Nerve Roots which enter into the Formation of the Lumbo-sacral Plexus of *Macacus rhesus.*" By J. S. RISIEN RUSSELL, M.B., M.R.C.P., Assistant Physician to the Metropolitan Hospital. Communicated by Professor VICTOR HORSLEY, F.R.S. Received March 22, 1893.

(From the Pathological Laboratory of University College, London.)

(Abstract.)

As the history of this subject was fully detailed in a former paper* by the author on the brachial plexus of the Dog, only such experimental work as has been done in connexion with the lumbo-sacral plexus is reviewed in the present communication.

In dealing with the anatomy of the Monkey it is shown that the class of plexus most commonly met with has many features in common with that described by Sherrington as the "prefixed" class of plexus; while of the variations met with that which occurred most frequently has many points in common with the class of plexus designated "postfixed" by that observer; but that there is one very notable difference between the last two, as in no instance was the 2nd sacral nerve root found to contribute a branch to the sciatic nerve, a contribution which Sherrington describes in this class of plexus.

Certain indirect effects brought about by muscles in connexion with joints on which they have no direct action, and the necessity for the exclusion of such indirect effects in the study of the movements at these joints, are discussed. The methods employed in operating are detailed, and the plan on which the results are arranged for description given.

Excitation experiments form the first part of the experimental portion of the paper; and, for convenience, the compound movements obtained by excitation of the whole nerve root are described in conjunction with the minute differentiation obtained by excitation of the individual natural bundles of the nerve roots. Following this is a description of the results of the direct observation (after dissection) of muscles thrown into action by excitation of the separate nerve roots. And as a corollary to this part of the subject, the question as to whether a single bundle of nerve fibres representing a single simple movement ever remains distinct in a nerve root during its course to the muscles which it supplies without inosculating with other motor nerve fibres is considered. The obvious necessity for control experi-

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* 'Phil. Trans.,' B, 1893.

ments led to the observation of the alteration in the action of the posterior extremity in progression, in climbing or in standing, evoked by section of one or more nerve roots. A second method of control consisted in the observation of the influence of section of a root or roots in excluding part of an epileptic spasm induced in the limb by intravenous injection of absinthe. As a corollary to this, the question as to whether the results differed in any way when the section of the root or roots was made some time previously, or at the time when the general convulsions were evoked, was tested. Special attention is called to the advantages of this method of experimentation, made use of by the author in a former research, but otherwise not yet adopted by other investigators.

Excitation Experiments.

In discussing the results obtained by this method of experimentation the discrepancies which exist between the results obtained by Ferrier and Yeo, by Sherrington, and by the author respectively, are pointed out. With regard to the upper limit at which nerve fibres leave the spinal cord for the supply of the lower limb, the first of these observers are held to have placed the limit too low, while Sherrington has placed it too high, the author finding that the 3rd lumbar root is the highest which supplies nerve fibres to muscles acting directly on the limb. The author agrees with Ferrier and Yeo in considering the 1st sacral nerve root the lowest of the series which contributes nerve fibres to the limb, and he has never found the 2nd sacral nerve root supplying the limb even in that class of plexus designated "postfixed" by Sherrington, in which, according to this observer, the 2nd sacral sends a branch to the sciatic nerve.

Great difficulty is experienced in attempting to reconcile Sherrington's results with those obtained by the author, as regards the number of nerve roots in which a given muscle is represented, and, conversely, the number of muscles and, in consequence, movements represented in certain roots; unless it be that Sherrington has included every variation, while the author has only included those roots in which a given muscle is most commonly represented and those movements or muscles most commonly found represented in any given nerve root.

The author does not think that the developmental processes which bring about the arrangement of nerve fibres do so on a purely anatomical basis, without regard for physiological combination; and arguments in support of this view are adduced.

Contrary to the observations of Sherrington, who found that each bundle of nerve fibres which contributes to the formation of a nerve root represents, as it were, a miniature root, containing nerve fibres

for the regulation of all the movements represented in the compound root, the author finds that each separate bundle of nerve fibres in a nerve root represents a single simple movement, and not all the movements of the compound root in lessened degree. The explanation offered for this difference in the results is that possibly Sherrington did not separate the bundles of nerve fibres from each other for a sufficient distance in their course, and thus did not effectually exclude the possibility of diffusion of the current to other bundles of nerve fibres contained in the same nerve root.

The single simple movements thus eliminated are found to bear an almost constant relation to the nerve roots, the same movements being as a rule found in any given root, and such movements always bear the same relation to the spinal level. Further, each bundle of nerve fibres representing a single simple movement in a nerve root remains distinct in its course to the muscle or muscles producing such a movement, without inosculating with other motor nerve fibres.

The group of muscles supplied by any given nerve root occupy both the anterior and posterior surfaces of the limb; in other words, muscles whose unimpeded action would produce one movement are represented in the same nerve root as others whose action would produce a movement diametrically opposite.

When a certain group of muscles are found to predominate in their action in one root, they as a rule predominate in that root. If those producing flexion at a certain joint predominate in their action in one nerve root, those producing extension predominate in another.

In those instances in which two opposed movements are represented in three consecutive nerve roots, the middle root of the series is that in which both movements are represented, while the root above contains the one movement, and that below contains the other.

As regards the order of representation of the movements of flexion and extension from above down, they are found to alternate, flexion being at a higher level than extension in the highest segment of the limb, while extension is above flexion in the next segment, and so on. And these results are found to agree with those obtained in connexion with the anterior extremity of the Dog, except in the case of the elbow joint in its relation to the knee.

It is found possible by stimulation of a single bundle of fibres in a nerve root to produce contraction of a single muscle, and of it alone; but this effect is not nearly so easy to obtain as in the case of the cervico-dorsal roots, owing to the distance between the points of exit of the roots from the neural canal and those where they unite to form the plexus being too short to allow of sufficient separation of

the nerve fibres so as to exclude the possibility of diffusion of the current.

The same muscle is represented in more than one nerve root, usually two, and to an unequal extent in these. And when variation is met with it is a rule that one of the nerve roots in which the muscle is represented is different, rather than that it is represented in more nerve roots.

When the same muscle is represented in two nerve roots, the muscle fibres innervated by one root are not innervated by the other, so that only part of the muscle contracts when a single root is excited.

Ablation Experiments.

Division of any given nerve root produces paresis of the group of muscles supplied by it, which paresis is temporary, nearly all of it being recovered from. The amount of paresis or paralysis produced is proportional to the number of nerve roots divided; and this again varies according to whether the roots divided are consecutive or alternate ones, the effect being much greater in the former than in the latter case. Such division of one or more nerve roots does not result in incoordination of the remaining muscular combinations represented in other nerve roots; the remaining movements are merely more feeble.

Exclusion of a certain Root or Roots during an Epileptic Convulsion in the Limb.

Division of one or more nerve roots produces alteration of the position of a limb during an epileptic convulsion, which altered position depends on the muscular combinations that have been thus thrown out of action. And the effect is identical when the root or roots are divided at the time when the convulsions are evoked and when they have been divided some weeks previously. No incoordination is produced in the remaining muscular combinations; and there is no evidence of overflow of the impulses which ought to travel down the divided root, into other channels through the spinal centres, so as to reach the muscles by new paths.

III. " A Further Minute Analysis by Electric Stimulation of the so-called Motor Region (Facial Area) of the Cortex Cerebri in the Monkey (*Macacus sinicus*)." By CHARLES E. BEEVOR, M.D., F.R.C.P., and VICTOR HORSLEY, M.B., F.R.C.S., F.R.S. Received March 22, 1893.*

(From the Laboratory of the Brown Institution, and from the Pathological Department of University College, London.)

(Abstract.)

In the paper of which this is an abstract the authors have completed the minute analysis of the movements elicited by excitation of the excitable (so-called motor) region of the cortex cerebri in the Bonnet Monkey (*Macacus sinicus*). The portions hitherto examined having been those in which the movements of the limbs were represented, the facial area was chosen for the present research. After an historical introduction and a description of the anatomy of the region investigated, the method of notation and record of results is discussed.

Considering that in this part of the cortex cerebri there is well-defined representation of movements of both sides of the body, the question of bilaterality of representation is raised, and attention directed to its importance. The analysis of the results obtained showed that there existed precise localisation for the movements of the individual portions of the face, even to that of half the lower lip.

The specialisation of the movements of the tongue was rendered easy of examination by employing the operative device of dividing the tongue in the middle line. This shed unexpected light on the representation of the movements of this organ.

Movements of the pharynx were made the subject of observation, and some degree of unilaterality was discovered in the movements of the soft palate.

Finally, attention is drawn to the fact that the marches of movements in succession are in this region very inconstant and difficult to arrange.

IV. "On the Influence exercised by the Central Nervous System on the Cardiac Rhythm, with an Inquiry into the Action of Chloroform on that Rhythm."[*]   By JOHN A. MACWILLIAM, M.D., Professor of the Institutes of Medicine in the University of Aberdeen.   Communicated by Professor M. FOSTER, Sec. R.S.   Received March 23, 1893.

The following is a brief account[†] of the main results obtained in the course of a prolonged investigation of the above subject.

The animals employed were chiefly cats and rabbits, anæsthetised or narcotised with chloroform or chloral hydrate.   In the great majority of the experiments, cats were used anæsthetised with chloroform ; and it is the results obtained in those circumstances that I am to be understood as specially dealing with in this paper.

The chief object of the present investigation was to examine more fully the mechanism through which changes in the pulse rate are effected and the nature of those changes, when such are dependent on an influence exercised by the central nervous system upon the cardiac rhythm, *e.g.*, the changes in the pulse rate induced by afferent impulses, &c.

In proceeding to examine the influence of the nervous system on the cardiac rhythm, it is necessary in the first place to determine the conditions depending on the presence of the anæsthetic, as far as they bear on the cardiac rhythm, the influence of chloroform upon the heart itself and upon the centres of the cardiac regulating nerves.

I. ON THE RELATION OF CHLOROFORM TO THE CARDIAC RHYTHM.

With a view to the elucidation of this question, I have carried out the following series of experiments, to test the action of chloroform under different conditions and so determine the manner in which it may affect the rhythmic mechanism of the heart :—

1. Experiments in which the entire cardiac regulating mechanism was intact.

2. Experiments after section of the cardiac augmentor nerves, the vagi being left uninjured.

3. Experiments in which the vagi were divided, the cardiac augmentor nerves remaining intact.

4. Experiments in which the whole of the cardiac nerves were divided, all direct connexion between the heart and the central nervous system being thus severed.

† Full details and references are stated in a longer paper, soon to be published.

The main results of these experiments I shall now state briefly :—

1. *Experiments in which the Entire Cardiac Regulating Mechanism was Intact.*

When chloroform is administered by inhalation in the usual way, two well-marked stages are usually evident in its action on the cardiac rhythm :—

A. A stage of acceleration far beyond the ordinary pulse rate of the animal. From the normal rate of 120—130 (cat), the pulse may rise as high as 240—250. The acceleration occurs during the general excitement induced by the anæsthetic.

After a time, as the administration of chloroform progresses, the acceleration diminishes and gives place to the second stage.

B. A stage of moderate or slow pulse rates. In this stage the cardiac rhythm falls towards the normal rate; it may come to be closely similar to the normal rate (120—130 per minute), or it may stand at a higher or a lower level, though within such limits as to render the pulse frequency a comparatively moderate one (90—150 per minute). This stage usually continues during deep anæsthesia, and even when the narcosis becomes very profound and dangerous.

These stages, as seen in the cat, closely resemble, in their general character, the similar phases usually recognisable during the administration of chloroform in man.

It should be mentioned that in some cases the reduction of pulse rate which follows the primary acceleration is comparatively slight, the frequency of the beat remaining high above the normal standard even during profound anæsthesia. The conditions determining such a difference from the usual course of events cannot be discussed here; they are considered in my detailed paper on the subject. In some other other animals (*e.g.*, the hare) the heart commonly beats with great rapidity (260, &c.) even during deep anæsthesia, and in this respect such animals differ notably from the cat and from man.

During a certain period of chloroform anæsthesia the cardiac rhythm may readily be altered to a very important degree—either in the direction of slowing or acceleration—by excitation of afferent nerves or by changes in the blood pressure, &c. The period alluded to comprises the latter part of the first stage or stage of acceleration and the earlier part of the second stage or stage of moderate or slow pulse rate. I do not wish to imply that at no other phase of chloroform administration *can* changes in the heart's rhythm be brought about by similar causes, but only that the period referred to is the one most favourable to their ready manifestation, and the one in which their action is most constant. In the latter part of the second stage the cardiac rhythm passes beyond the influence of afferent impulses.

It need hardly be remarked that if strong chloroform vapour be too suddenly administered, well-marked reflex slowing of the heart may occur, as is well known; and such slowing may precede what I have described as the first stage or stage of acceleration.

Evidence as to the mode in which chloroform brings about the alterations in the cardiac rhythm which have been mentioned is afforded by the results of the subsequent series of experiments.

2. *Experiments after Section of the Cardiac Augmentor Nerves, the Vagi being left Uninjured.*

Those nerves were divided by an operation in which resection of the two uppermost ribs was performed, artificial respiration being maintained; the inferior cervical and the 1st thoracic (stellate) ganglia, together with the annulus of Vieussens, were completely removed on both sides, their connexions with the spinal nerves and the vagi being followed up for a considerable distance and divided. The sympathetic chain was cut through about 2 cm. above the inferior cervical ganglion, and about the same distance below the ganglion stellatum. The vagi themselves were left intact.

No blood vessels were tied, and there was nothing beyond a very inconsiderable loss of blood in the whole proceeding.

When the influence of the cardiac augmentor nerves has been excluded in this way there is no essential change evident in the action of chloroform upon the cardiac rhythm; the stage of acceleration (A) and that of moderate or slow rate (B) are still plainly apparent during the lighter and the more profound phases of anæsthesia. It is clear, then, the augmentor nerves are not the essential channels through which chloroform produced its important effects upon the pulse rate.

3. *Experiments in which the Vagi were Divided, the Cardiac Augmentor Nerves remaining Intact.*

The results obtained in this set of experiments were so essentially similar to those of the following group that the two may be taken together. The presence of the intact augmentor nerves does not seem to be of any essential importance as far as the effect of chloroform on the cardiac rhythm is concerned.

4. *Experiments in which all the Cardiac Nerves were Divided, all Direct Connexion between the Heart and the Central Nervous System being thus Severed.*

In this condition the heart beats very rapidly, being liberated from the controlling influence of the cardio-inhibitory centre in the medulla.

The rate of action rises far above the normal, provided the blood pressure remain good, and the temperature and general condition of the animal continue satisfactory ; the pulse rate rises from a normal rhythm of 120—130 to one varying from 216 to as high a figure of 240—250, according to the depth of the anæsthesia and other circumstances.

When the heart is beating in this very rapid fashion the further administration of chloroform deepening the anæsthesia, induces, as a rule, a very distinct reduction in the pulse rate, while at the same time the blood pressure falls markedly. The reduction in the pulse rate is not at all comparable in extent to that which usually follows a similar dose of chloroform while the vagi are intact : the diminution in rapidity in the present case (when the vagi or all the cardiac nerves are cut) is comparatively a slight one, *e.g.*, a reduction from the rate of 240 to one of 200, &c.

But, though the slowing of the rhythm is a much less extensive one than occurs while the vagi are intact, it is at the same time a very appreciable and constant one; and, moreover, it is important, depending as it evidently does on some cause affecting the heart itself, and not exerted through the influence of the regulating (extra-cardiac) nerves.

Now, as to the mode in which this slowing effect is produced in the action of the cardiac mechanism, the possible influence of the markedly-lowered blood pressure has to be taken into account. We know that in certain circumstances a great lowering of the blood pressure may lead to a very pronounced slowing of the pulse rate, and the question arises as to whether such a cause is sufficient to explain the reduction in rate caused by chloroform in the conditions now under consideration. I conclude that it is not, for these reasons :—

1. The diminution in the rate of heart beat may begin *early* when the fall of pressure is only slightly developed, and

2. A similar, or indeed a much greater fall of blood pressure lasting for similar periods, when induced by mechanical causes (*e.g.*, compression of portal vein, &c.), entirely fails to cause any diminution of rate like that which occurs when the pressure is lowered by chloroform. Experiments in which reduction of blood pressure of equal amount and duration were alternately induced by (*a*) chloroform and (*b*) mechanical causes have clearly shown this.

There is good reason to believe that chloroform has a special influence on the rhythmic mechanism of the heart, an influence not dependent on the concomitant lowering of the blood pressure caused by the drug.

Again, as to the nature of the influence exerted by chloroform in this way, it can be shown that the slowing is not dependent on a

stimulation of the local inhibitory mechanism of the heart. For the administration of atropia in doses sufficient to paralyse that mechanism does not obviate the slowing effect of the anæsthetic. (The paralysed condition of the inhibitory mechanism was verified by strong electrical stimulation of the inhibitory area, described in former papers in the ' Proceedings of the Royal Society,' vol. 44, and the ' Journal of Physiology,' vol. 9, 1888.)

From these results it appears that chloroform acts on the heart, and distinctly slows its rate of beat through a depressing or retarding influence exerted on the intrinsic rhythmic mechanism of the organ.

Further, it is evident that the reduction in the pulse rate that occurs in the second stage (B) of chloroform anæsthesia is of a two-fold origin : it partly results from the action of chloroform on the heart itself, but it very largely depends on the integrity of the vagi. The vagus centre in the medulla, though rendered incapable of reflex excitation, is not paralysed even during deep anæsthesia; it continues, as a rule, to exert a very important controlling influence upon the cardiac rhythm, and it is indeed to the exercise of this controlling influence that the reduction of the pulse rate from the excessively high rapidity often present during the stage of acceleration is mainly due. Even during profound anæsthesia, section of the vagi leads, as a rule, to a striking acceleration of the pulse rate, though the rapidity does not become as great as during a lighter anæsthesia. When the anæsthesia is allowed to become less deep the rate of heart beat increases decidedly.

As regards the notable acceleration of the heart which occurs in the first stage (A) of chloroform administration (stage of excitement), this is not to be accounted for by an assumed excitation of the cardiac augmentor nerves, for marked acceleration occurs under the influence of the anæsthetic even when the augmentor nerves have been excluded by section.

On the other hand, the action of chloroform at different stages of its administration on the heart itself after section of all the cardiac nerves shows no ground for assuming that the acceleration might be due to direct influence on the intrinsic rhythmic mechanism.

Moreover, the general indications of excitement in the central nervous system (respiratory centre, &c.) point to the probable occurrence of central changes in the centres of the regulating nerves. And as an assumed activity of the augmentor nerves has been set aside as inadequate to meet the facts, there remain only the vagi. A reduction in the controlling influence of the vagus centre would cause such an acceleration as that which usually occurs, and it is, I believe, to such a change that the quickened pulse rate is mainly, if not entirely, due.

This change—the diminution in activity of the vagus centre—is

not due simply to a fall of blood pressure; it occurs while the blood pressure is still high, and shows no constant relation to alterations in the pressure, sometimes occurring when the blood pressure is rising, and sometimes when it is falling, and at other times when there is no important alteration of level.

The cause of the alteration of phases of slowing and acceleration is to be sought in an alternately increased and diminished activity of the vagus centre. Further evidence regarding the occurrence of such variations in the conditions of the centre will be adduced later in this paper.

In those cases alluded to—when the heart continues to beat at a rapid rate even during profound anæsthesia—the cause appears to lie mainly in a continued suspension of the activity of the vagus centre, a great diminution or a removal of its controlling influence on the heart. In this condition chloroform may still slow the heart, though only to a relatively slight extent, by its direct influence on its intrinsic rhythmic mechanism of the organ.

II. On the Relation of the Heart's Rhythm to Changes in Blood Pressure.

It is well known, since the work of Ludwig and Thiry upon the subject, that changes in blood pressure may affect the rate of the heart's beat otherwise than through the mediation of the cardiac regulating nerves.

Many investigators have dealt with this relation, with wide variations in the results arrived at; and such variation is not surprising when one considers the different conditions that may be present in such experiments, differences as to the animals used, the drugs (curare, chloral, morphia, &c.) administered, the various levels from which the change of blood pressure may start, the duration of such pre-existing level, the character and duration of the change itself, the conditions as regards intra-cardiac tension (in one or more parts of the organ) attendant on changes in the arterial pressure, &c.

In view of such circumstances, I have found it necessary to perform a considerable number of experiments to demonstrate how far the cardiac rhythm was affected by blood pressure changes in such conditions of experiment as those commonly present in the present investigation (simple chloroform anæsthesia, &c.), especially as regards the competency of more or less sudden blood pressure changes of short duration to account for important changes in the rate of the heart's beat.

Such experiments I have performed in the following conditions:—

A. *When the heart was released from all direct influence which may be exercised through the cardiac regulating nerves.—By section of all*

these nerves, or by destruction or death of the medulla and cervical spinal cord (cat and rabbit).

The general results obtained were these :—*a*. In this condition it was found that the existence of an extremely low pressure continuing for some time (minutes) leads to a very pronounced slowing of the pulse rate, and a subsequent elevation of the pressure may cause a more or less extensive acceleration of the beat.

b. An exceedingly high blood pressure may cause marked slowing of the heart through its influence on the intra-cardiac mechanism.

c. When the blood pressure is at a fair height, the occurrence of a great fall of pressure lasting for short periods (*e.g.*, 30 seconds) causes no change in the cardiac rhythm.

Nor does a rise of pressure from this low level—established as it has been for only a brief space of time—to the preceding level, or even a good deal higher, involve any appreciable change in the pulse rate.

A similar negative result is present when, instead of undergoing a fall from the original level, the pressure is made to rise ; there is no change in the pulse rate unless the rise attains to such proportions as to induce the slowing mentioned in (*b*).

There can be no doubt that extensive variations in the blood pressure (*e.g.*, between 30—40 mm. and 150—160 mm.) if of brief duration do not as a rule involve any important or constant change in the rate of the heart's beat.

B. *When all the Cardiac Nerves were Intact.*—The results here obtained were in complete accordance with those usually described—a raised blood pressure causing well-marked slowing of the heart, and a fall of blood pressure carrying with it a notable acceleration—in the absence of disturbing or complicating causes.

It may be remarked that similar changes are seen after the cardiac augmentor nerves have been divided; they are evidently essentially due to changes in the activity of the vagi.

It may also be noted that the *continuance* of an excessively low pressure will ultimately lead to a great slowing of the pulse rate—following on the primary acceleration—in virtue of the influence of such a condition of blood pressure on the cardiac rhythm already mentioned (A, *a*), so that an extreme and prolonged lowering of the blood pressure occurring when the heart is beating at an approximately normal rate causes first a remarkable acceleration of the pulse, which ultimately gives place to a slow rate of beat. The latter change may be counteracted by an abnormally high temperature if such is present.

III. On the Effects of Excitation of Afferent Nerves upon the Cardiac Rhythm.

The changes excited in the cardiac rhythm as a result of stimulation of afferent nerves in favourable circumstances may be either of the nature (A) of a slowing or (B) an acceleration of the heart beat; whether the former (A) or the latter (B) takes place varies according to the conditions present at the time—the particular nerve stimulated, the strength and suddenness of the stimulation, the exact degree of chloroform anæsthesia, and the state of the medullary centres in other respects.

Some Features in the Results of Stimulation of Afferent Nerves.

1. Speaking generally, excitation of visceral or splanchnic afferent nerves (*e.g.*, vagus, cervical sympathetic, abdominal splanchnic, &c.), is more readily effective in altering the cardiac rhythm under chloroform than excitation of somatic afferent nerves. The result may be acceleration or slowing, or an alternation of these, in the case of either class of nerves.

2. Moreover, there is a very important difference between the action of the splanchnic and that of the somatic afferent nerves upon the pulse rate. The acceleration resulting from a stimulation of somatic nerves is accompanied by signs of diffuse motor excitation as indicated by the occurrence of more or less general muscular contraction, whereas acceleration often results from stimulating splanchnic or visceral nerves when there are no concomitant movements and no sign of any association with general motor excitation at all. In curarised animals, such as have been used by the great majority of investigators, this difference would not of course be evident.

3. Again, strong sudden stimulation of an afferent nerve may cause marked slowing, while weaker and more gradual stimulation of the same nerve causes acceleration. *Cæteris paribus*, strong sudden stimulation is relatively more apt to cause slowing. (Of course this does not necessarily hold good with *natural* stimulation of various afferent nerves.)

4. Further stimulation of such nerves (*e.g.*, the brachial or intercostal) may cause notable slowing of the heart in suitable conditions of anæsthesia (in the period already referred to as the most favourable one); but they commonly cause acceleration as their *only* effect on the cardiac rhythm when the anæsthesia is rendered somewhat deeper; and at a still later phase the afferent excitation induces no change at all in the pulse rate. An excitation which causes slowing, followed by acceleration, of the heart, at a certain phase of anæsthesia, commonly fails to cause any slowing at all, but only acceleration—if any effect at all—when more chloroform has been given.

Mechanism through which Changes in the Cardiac Rhythm are induced by Afferent Impulses.

A. *Reflex Slowing.*—The essential mechanism of reflex slowing is clear enough, the change being due, as is commonly believed, to an increased activity of the vagus centre; it does not depend on alterations in the blood pressure, or asphyxial conditions, or on any influence of the augmentor nerves, for it may be readily excited in the usual way after these nerves have been cut.

B. *Reflex Acceleration.*—It is evident that this change might be due to an alteration in the regulating influence exercised by the central nervous system on the heart through the cardiac nerves (vagi and augmentors), or to changed conditions induced in the heart itself, or to a combination of such causes.

Influence of the Cardiac Augmentor Nerves and the Vagi.

I have come to the conclusion that the augmentor nerves do not play the essential part, *i.e.*, that the reflex acceleration is not essentially dependent upon excitation of the augmentor nerves.

The chief evidence on this point may be briefly stated as follows :—

1. *The latency and character of the acceleration which often results from excitation of afferent nerves may be entirely different from what has been shown by many observers to be characteristic of the action of the augmentor nerves.*—Stimulation of the latter is followed, as is well known, by a long latent period, and then a quickening or rhythm begins, and gradually increases to a maximum (5, 10, or 15 seconds after the beginning of the stimulation, usually) ; later, the acceleration declines and gradually disappears. But when an afferent nerve is excited, the resulting cardiac acceleration not unfrequently presents itself after an exceedingly short and hardly appreciable interval, occurring with remarkable suddenness, and at once, or almost at once, attaining a very high value, or, it may be, its maximum. Such cases, though by no means the most common ones as regards the results of afferent excitation, are sufficiently numerous and unmistakable to be highly significant in the present connexion, showing, as they do, features in the change of rhythm which differ most strikingly from those characteristic of the action of the augmentors, and which, on the other hand, correspond most closely with what occurs when the controlling influence of the vagi is diminished or removed.

In some cases the extensive acceleration which may follow stimulation of afferent nerves sometimes, after lasting for a variable period, suddenly gives place to a phase of slow heart beat, the reflex acceleration vanishing as abruptly as it had begun. A more or less rhythmical alternation of suddenly recurring phases of acceleration

and slowing may at times be seen, and strongly suggests the occurrence of corresponding variations in the activity of the vagus centre as their cause.

An assumed excitation of the augmentor nerves could not afford a probable explanation of these facts; and the evidence points very decidedly to changes in the activity of the vagus centre as the primary and paramount cause of both the slowing and the acceleration reflexly excited in the way referred to.

2. *The result of stimulation of afferent nerves after section of the vagi while the cardiac augmentor nerves remain intact.*—Section of the vagi in the cat leads to a great acceleration of the cardiac rhythm, the pulse rate rising to a maximal rate of 216—250; the maximum varying to some extent in different animals and in different circumstances.

In this condition, the augmentor nerves being uninjured, excitation of afferent nerves gives rise to no further acceleration. But this does not constitute any proof as to whether the augmentor nerves are reflexly excited by afferent stimulation or not. For the circumstances are not favourable for augmentor excitation being effective in causing any notable change in the cardiac rhythm. It is, indeed, quite clear, as shown by direct stimulation, that the augmentors *can* accelerate the heart after section of both vagi, *e.g.*, when the blood pressure is extremely low, and the cardiac rhythm slowed in consequence; their action is not confined to a counteracting of the controlling influence of the vagi upon the rhythmic mechanism. But when the vagi are cut, and the heart beats at the high maximal rate mentioned above, the blood pressure remaining high and the bodily temperature, the respiration, &c., being satisfactory, direct stimulation of the augmentor nerves is without any appreciable accelerating effect upon the pulse rate. In such circumstances then, when the heart is beating at its high (maximal) rate, the non-occurrence of acceleration in response to excitation of afferent nerves cannot be taken as evidence that the cardiac augmentor centre is not reflexly affected by afferent impulses.

It is necessary, therefore, to reduce the cardiac rhythm to a lower level, at which it might be expected (judging from the results of direct stimulation of these nerves) that a sudden reflex excitation of the augmentor nerves would be able to manifest itself by an appreciable effect on the pulse rate, if the changed pulse rate (acceleration) induced by afferent excitation is really due to an excitation of the augmentor nerves.

The heart may be kept beating at a moderate rate, the normal rate, or somewhat slower or faster than normal, with much regularity for periods of some length by slight continued stimulation of the vagus nerve in the neck, after both these nerves have been cut. When this

is done, an afferent nerve (which was found to give marked reflex acceleration as long as the vagi were intact) can now be stimulated as before, with the result of causing evident manifestations of general motor excitement, shown by the occurrence of marked convulsive movements; but in this case there is no cardiac acceleration. The moderate rhythm maintained by slight continued excitation of the vagus is not appreciably interfered with by stimulation of afferent nerves; and this negative result I have obtained in every experiment with complete constancy. It strongly opposes the idea that the sudden and extensive acceleration frequently seen on stimulating afferent nerves is due to a reflex action of the augmentor nerves; for, as soon as the vagi are cut, no reflex acceleration occurs, even when, as in these experiments, the heart was kept beating at a moderate rate, a condition favourable to the marked manifestation of the influence of augmentor excitation upon the cardiac rhythm.

3. *The results of excitation of afferent nerves, after section of the cardiac augmentor nerves, the integrity of the vagi not being interfered with.*—The augmentor nerves were cut by a mode of operative procedure already stated. That the vagi remained uninjured was shown by their being readily excited through a rise of blood pressure or by a suitable stimulation of certain afferent nerves, well marked slowing of the heart being evident in each case.

In this condition, the cardiac augmentors being now excluded, stimulation of afferent nerves, which caused acceleration prior to section of the augmentors, can still cause a very marked quickening of the pulse rate. In the case of the brachial or intercostal nerves, for example, stimulation (mechanical or electrical) leads, in favourable circumstances, to a very decided change in rhythm, provided as before that the stimulation is also effective in causing general motor excitation; an acceleration is speedily induced, frequently to the extent of 20 per cent., but sometimes much more.

In some cases a brief phase of cardiac slowing is also seen; and in other instances a more or less regular alternation of phases of slowing and acceleration presents itself. The same general relation already noted as obtaining between the strength and suddenness of the stimulation, the excitability of the medullary centres (depending on depth of anæsthesia), and the nature of the change in rhythm as regards the occurrence of slowing or acceleration, is again observable here; also the notable difference between the splanchnic and somatic nerves, as regards the constant association of general motor excitement with the reflex cardiac acceleration which occurs in the latter case, and the frequent absence of any such association in the former.

In short, the action of afferent excitation upon the cardiac rhythm, as far at least as its main features are concerned, is essentially similar whether the augmentor nerves are divided or intact.

It will be seen that the evidence yielded from these three different sources is in complete accord—the evidence derived from (1) a consideration of the nature of the cardiac acceleration which may occur, (2) the negative results obtained after section of the vagi, and (3) the (complementary) positive results seen in unmistakable form after exclusion of the augmentors while the vagi remain intact.

Possibility of Changes in the Peripheral Efficiency of the Vagi.

It remains to be seen how far a diminution of the controlling influence exercised by the medulla on the cardiac rhythm might be due to changes in the heart itself—changes leading to an impairment of the peripheral efficiency of the vagi, *e.g.*, the influence of the products of muscular contraction, or of altered conditions of intra-cardiac tension, especially in the right heart, depending on the occurrence of convulsive movements, &c.

As far as the splanchnic afferent nerves are concerned, these causes may be set aside, for, as has been already stated, reflex acceleration frequently occurs without any contraction of the skeletal muscles.

In the case of the stimulation of the somatic afferent nerves (where muscular contraction does occur) it is quite clear that the causes mentioned above are also unessential. Reflex acceleration sometimes occurs with an amount of muscular contraction far too slight to cause any such sudden alterations in either of the ways suggested ; and, more, it may occur even after the skeletal muscles have been paralysed by curare. It has been already stated that there is no constant relation between the changes in blood pressure (frequently slight in amount) and the occurrences of reflex acceleration.

Further, I have examined the relation borne towards the peripheral efficiency of the vagi by (*a*) changes in arterial and intra-cardiac pressure and (*b*) well marked convulsive movements excited by stimulation of afferent nerves, after all the cardiac nerves have been cut, or the medulla destroyed or killed (cat or rabbit). The results have been as follows :—

a. Great and sudden changes in arterial pressure (*e.g.*, between 40 and 150 mm.) induced by compression of the descending thoracic or the upper part of the abdominal aorta, closure of the vena porta, &c., for short periods (*e.g.*, 30 secs.) though frequently accompanied by great changes in intra-cardiac tension had little or no appreciable effect upon the maintenance of the moderate or slow rate of beat maintained by slight continued excitation of the vagus in the neck.

b. The occurrence of very marked convulsive movements in response to stimulation of afferent nerves does not, even after several seconds, cause any perceptible change in the slowing influence of slight continued excitation of the vagus.

It is obvious that, whatever effects a *continuation* of altered intra-cardiac tension or muscular action may have upon the peripheral efficiency of the vagi, those causes are not in any way capable of accounting for such *sudden and extensive* reflex acceleration as is frequently seen when an afferent nerve is stimulated.

The primary and essential cause of the altered cardiac rhythm is clearly to be sought elsewhere, and it is obviously to be found in an altered activity of the central vagus mechanism. The acceleration depends on a diminution in activity of the vagus or cardio-inhibitory centre; just as the cardiac slowing is due to an increased activity of the same centre.*

Influence of Muscular Exertion upon the Cardiac Rhythm.

The striking association of cardiac acceleration with motor effort following excitation of afferent nerves distinctly suggests the existence of a somewhat close connexion between the motor mechanism and the cardiac regulating centres in the medulla. As has already been stated, the essential connexion is with the vagus centre; a diminished activity of that centre is associated with motor effort and leads to an accelerated heart beat.

And there is much to be said in favour of the hypothesis that a similar lessening of the controlling influence exercised by the vagus centre on the heart occurs during muscular exertion and constitutes at least *one* of the causes of the characteristic quickening of the pulse.†

It may be noted that in certain conditions of impaired health the heart becomes accelerated with extreme readiness from exertions, a very brief interval elapsing between the commencement of muscular actions and the change in cardiac rhythm. In some such instances it is difficult to find a feasible explanation either in an assumed influence of the products of contraction or in a supposed excitation of the augmentor nerves.

It is interesting to note that animals endowed with great staying powers (dog, horse, &c.) have, in many cases at least, a comparatively slow pulse rate in the quiescent condition; and that the heart is capable of an immense relative increase in the rate of its action as seen during muscular exertion or on section of the vagi. The con-

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* Some recent observers (Roy and Adami, 'Phil. Trans.,' 1892) have assumed that the cardiac acceleration resulting from stimulation of afferent nerves may be taken as an indication of the action of the augmentor nerves on the heart. The untrustworthiness of this assumption—in the light of the results stated above—need hardly be commented on.

† It is hardly necessary to remark that *various* influences are probably concerned in determining the pulse rate during ordinary muscular exercise.

ditions present, in this connexion, in two such animals as the hare and the rabbit are very noteworthy. These animals, though very closely allied, from a zoological point of view, live under very different functional conditions in certain respects : the rabbit is able to run *short* distances with great rapidity, but not to traverse *long* distances without intermission—this being no doubt in relation to the fact of their having burrows to flee to; the hare, on the other hand, destitute of such means of protection, has to depend, in the open country, upon its endurance in swift locomotion. On examining the cardiac rhythm of this animal, I find it presents a remarkable contrast to that of the rabbit. In the latter animal, section of the vagi, as is well known, causes comparatively little acceleration. In the hare, on the other hand, in which the normal pulse-rate is much slower (*e.g.*, 64 per minute), the heart beats at an enormously increased rate (*e.g.*, 264 in the same animal) after the vagi have been divided.

In this animal, while the vagi are intact, the cardiac rhythm shows very sudden and extensive changes when the animal is disturbed, &c. These changes are probably due to alterations in the activity of the vagus centre.

Influence of Emotion upon the Cardiac Rhythm.

The characteristic acceleration of the heart induced by emotion in animals and in man is probably due, largely or mainly, to a diminished activity of the cardio-inhibitory centre. The acceleration is sometimes preceded by a phase of slowed action, due to excitation of the same centre, and the entire effects present a very striking resemblance to those induced by afferent impulses. This correspondence strongly suggests that the effects are, in the two cases, primarily dependent on similar causes—changes in the activity of the vagus centre—the primary slowing being due to a heightened activity of the centre and the subsequent acceleration to a reaction in the way of diminished activity—resembling what may be seen in certain other centres under the influence of emotional conditions. The suddenness and extent of the alterations in cardiac action sometimes induced by powerful emotion are easily applicable in connexion with such a view.

Conclusion.

From the evidence available on the subject, there is good reason to believe that, in addition to the well-known influences of blood pressure, asphyxial blood, &c., upon the vagus centre, the activity of that centre is affected either in way of increase or diminution by the play of afferent impulses from various parts, by impulses

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(emotional, &c.,) from the higher parts of the central nervous system, and probably also, as far as a diminution of its activity is concerned, by the influence of some portion of the nervous mechanism concerned in the execution of muscular effort. And, as far as we know, it appears to be mainly upon the mediation of the vagus centre that the most important changes in the cardiac rhythm, in so far as determined by nervous influences, are primarily dependent.

V. "On the presence of Urea in the Blood of Birds, and its bearing upon the Formation of Uric Acid in the Animal Body." By Sir ALFRED GARROD, M.D., F.R.S. Received May 2, 1893.

Some experiments upon which I have recently been engaged have yielded results which appear to be inconsistent with any explanation yet advanced of the mode of formation of uric acid in the animal body, and to necessitate a new theory of its formation. These results I propose to discuss in the present paper, but before doing so it will be well to refer briefly to the different views upon this subject which have been from time to time advanced.   ♦

Until the year 1847 it was commonly supposed that uric acid was formed in the kidneys themselves, none having ever been detected in the blood, but in that year I succeeded in demonstrating the presence of uric acid in the blood of gouty subjects, which led to the conclusion that uric acid was formed in certain other organs and tissues of the body, and was merely eliminated by the kidneys.

It is now generally supposed that in mammals urea is produced as the ultimate product of the metabolism of nitrogenised tissues, the formation of a soluble urate being an intermediate stage of the metabolism; but that in birds the nitrogen is eliminated in the form of urate of ammonium without having undergone the further change into urea. Many efforts have been made to explain why these changes should be so different in animals of these two classes. A view once very popular and supported by great authority was that the difference could be accounted for by the amount of oxidation in the system; for example, it was assumed that, as respiration in the cold-blooded animals is slow and imperfect, the uric acid is not broken up, but is eliminated in the form of urate of ammonium; whereas in hot-blooded animals, such as the mammalia, the oxidation processes are more active and urea is produced. Those who held this opinion had in mind only the mammal and the reptile; altogether overlooking the fact that the bird, which throws out uric acid in the same way as the reptile, has even hotter blood and a more active respiration than the mammalia themselves. Hence this view had to be aban-

doned, and the difficulty of explaining the above-mentioned changes remained as before.

Another explanation was that the occurrence of urea or uric acid is dependent on the character of the food, and even at the present time it is often assumed that a highly nitrogenised food, such as meat, leads to the production and elimination of a large amount of uric acid. The fallacy of this I showed in a paper read before this Society in 1886, in which I proved conclusively that mammals living on raw meat threw out infinitely less uric acid in proportion to their weight than small birds fed on canary seed and water. In that paper it was shown that the lion and the tiger eliminated probably less than a hundred thousandth part of their weight of uric acid per diem, whereas a small bird would eliminate as much as an eighty-fifth of its own weight. The amount of uric acid cannot, therefore, depend on the mere nature of the food.

There are, however, physiologists who do not consider it necessary to assume that urea must previously have existed as urate of ammonium, and some few have hinted that there may even be a synthesis in the production of uric acid. The whole subject is, however, in a state of great uncertainty, and, as Dr. Michael Foster has stated in his ' Text-book of Physiology,' " The whole story of proteid metabolism consists at present mostly of guesses and of gaps."

The difficulties in connexion with the formation of uric acid in the animal body have been before my mind for many years, and the aim of this paper is to suggest a solution which, in the first place, accords with facts which have been long established, and, in the second place, with others which I have recently discovered and proved experimentally.

It has always been difficult to me to believe that the metabolism of nitrogenised tissues should differ so completely in animals which are so closely allied in most other respects as the toad and the lizard, but which excrete their nitrogen, the one as urea, the other in the form of urate of ammonium, their diet being identical, and I think it will be found that the assumption that urea must be a product of the oxidation of urate of ammonium, has had much to do with the difficulties which have arisen in connexion with this subject.

Let us now consider certain points in the physiology of urea and uric acid, beginning with urea.

In mammalia, including man, it has been ascertained that urea is always present in the blood, and, though the quantity may be small, it is sufficient, nevertheless, to be measurable. I have recently obtained it not only from the blood of man, but also from that of the ox, the sheep, the goat, and the dog. And I have also confirmed a statement made by M. Picard that the blood of the renal artery is

much richer in urea than the blood of the renal vein, in the proportion, according to my experiments, of about three to one; of about two to one according to the experiments of M. Picard.

As far as I am aware, urea has never hitherto been detected in the blood of birds. Possibly it has never been looked for, as it would scarcely have been imagined that urea could be contained in the blood of an animal excreting only urate of ammonium. I have, however, recently found that the blood of birds contains urea in quantities not less than those found in the blood of mammals. The views I entertained on the formation of uric acid in the animal body led me to investigate this subject, as they necessitated the presence of urea in the blood of the uric-acid-excreting animals, as well as in the blood of those which eliminate urea only.

As the question of the existence of urea in the blood of different animals is of so much importance in connexion with the subject of this communication, I have taken special pains to determine not only that it is present, but also in what relative amounts, and the methods I have made use of have been as follows :—

In all my experiments I made use of dried blood, which form was by far the most convenient for my purpose. It was thus obtained :— Wide-mouthed bottles were half filled with absolute alcohol, and the blood as it flowed from the animal fell directly into the bottle, after which the spirit and the blood were intimately mixed by shaking. This mixture was then dried on a water-bath, reduced to a very fine powder, and kept in air-tight bottles. In making experiments, I assumed that one part of dried blood equalled five parts of liquid blood.

To determine the amount of urea I adopt the following method. The dried blood is treated with about six times its weight of distilled water, very slightly acidulated with oxalic acid, and this is heated in a water-bath to about 75° C. for at least six hours. The next day the residual blood is again treated in the same way with fresh acidulated water and again heated. The fluids poured off are then filtered and evaporated to dryness. Any urea abstracted from the blood is now in the form of the sparingly soluble oxalate of urea. If any oily matter is found in the residue of the evaporated fluids, this can easily be removed by washing with potassium naphtha, in which the salt of urea is totally insoluble. The residue is next treated with distilled water and a small quantity of the carbonate of barium (sufficient to cause a slight alkaline reaction) is stirred up with it. By this means the urea is set free and the oxalic acid fully neutralised.

After again evaporating to dryness, the residue is boiled with absolute alcohol, by which means the urea (together with small traces of other substances) is dissolved; and when this has been filtered into a large watch glass and evaporated spontaneously the urea is obtained *in a solid form.*

If this is again dissolved in distilled water and the solution introduced into a small ureometer filled with the solution of hypobromite of sodium, the quantity of nitrogen given off can be readily determined and the amount of urea in the solution can be deduced from it. 1 gramme of urea equals 0·46 gramme of nitrogen, or 372·7 c.c. In practice, however, 354·3 c.c. have been obtained.

If, on the other hand, we wish to crystallise the salt and directly weigh the amount, this is best effected by adding a few drops of nitric acid to the urea solution, so causing the formation of nitrate of urea. This can be weighed, after purification, by redissolving it in absolute alcohol.

The fact that the residue left after evaporation is undoubtedly nitrate of urea is proved by the characteristic form of the crystals (either simple rhomboids or aggregated varieties of these), or by a microscopic test which I have adopted of adding a drop of the hypobromite of sodium to the crystal under the microscope, and seeing the innumerable bubbles of nitrogen which are given off.

The following table shows the amount of urea found in a hundred parts of the blood of different mammals and birds:—

Sheep....	0·029 per cent.	Fowl	0·025 per cent.
Sheep....	0·025 „	Turkey ...	0·026 „
Ox	0·022 „	Duck	0·029 „
Ox	0·021 „	Turkey ...	0·024 „
		Fowl.....	0·026 „
		Duck	0·020 „
		Fowl.....	0·026 „
		Goose	0·020 „
		Turkey ...	0·022 „

It will be seen that, if we compare these results, the amount of urea in the blood of the mammal and of the bird is practically identical.

It has been found that in normal human blood the urea varies from 0·020 to 0·040 per cent. My results, obtained from the different animals above mentioned, are all within these limits.

When urea is introduced into the stomach of a mammal, it is eliminated by the kidneys in an unchanged form. This has repeatedly been shown to be the case, and the fact is not questioned by physiologists. On the other hand, when urea is introduced into the stomach of a bird, or other uric-acid-excreting animal, it is not thrown out in the form in which it is introduced; but there is, instead, an increased formation of uric acid. This has been shown to be the case by H. Meyer and M. Jaffe.[*]

Next with regard to uric acid.

* In the 'Berichte d. Deutschen Chem. Gesellschaft,' vol. 10, 1877, p. 1930.

Uric acid is not found in the blood of healthy mammals. Man, however, is, to some extent, an exception to this rule, for uric acid is often found when the deviation from good health is scarcely perceptible. This I showed in a paper in the 'Medical Chirurgical Transactions,' in 1848. Observations are usually made on the blood of animals which are killed for food, and these are probably young and healthy; whereas, when observations are made on man, the chances of his being in perfect health are diminished, since it is usually illness which brings his condition under notice.

The blood of healthy birds, instead of containing uric acid, as would naturally be supposed, is usually quite free from that substance, except when it has been introduced into the system through the stomach, or by injection. In a paper read before this Society in 1884, I showed that the blood of the duck was, for the most part, free from any detectible uric acid, and, in a paper read in 1848, I showed that the blood of a healthy pigeon was entirely free from it. I have recently had occasion to examine most minutely the blood of · the goose, the fowl, and the turkey, and have found that, although fairly rich in urea, no uric acid could be separated from it. We may, therefore, conclude that birds may throw out uric acid by the kidneys, although it is not present in their blood.

Perhaps it will be desirable to mention here that uric acid introduced into the stomach of the bird as a soluble urate is absorbed into the blood, which becomes so saturated with it that it can be crystallised out with the greatest ease.

Uric acid thus introduced into the system is not thrown out by the kidneys, as these organs appear to be incapable of eliminating it, and this applies not only to the bird but to the mammal also. This fact has been proved by the researches of Zabelli.

Having considered these facts, the correctness of which is capable of being fully established, we are justified, I think, in coming to the following conclusions :—

First. That in mammalia and other urea-excreting animals the metabolism of the nitrogenised tissues results in the formation of urea as an ultimate product; that an appreciable and measurable amount of this substance is always found in their blood, and is constantly being excreted by the kidneys; and, further, that any cause leading to the decrease of this excretion produces an augmentation of the urea in the blood. It necessarily follows from this that, as stated above, the blood of the renal artery is richer in urea than the blood of the renal vein.

Second. That in birds and other uric-acid-excreting animals the metabolism of the nitrogenised tissues is exactly the same as in mammals, and that urea is the ultimate product of this metabolism; *that* urea is always present in their blood, in quantities not less than

in mammalian blood, and that the urate of ammonium is a subsequent product of the union of urea with some other principle or principles, glycine probably being one of them. Consequently, it is not necessary that uric acid should be present in the blood of uric-acid-excreting animals: in health, in fact, it is not detectible. When it is present, its presence is a result of its having been absorbed after formation in the kidneys or elsewhere.

With regard to the first proposition but little need be said, as most physiologists will agree with the statements it contains. With regard to the second, the case is very different, as the views therein enunciated are totally at variance with those which are held at the present time. The statement that the ultimate metabolism in the uric-acid-excreting animals is identical with that which takes place in mammals requires for its establishment the constant presence of urea in their blood. And I was led to seek for urea by my views on the production of uric acid, and from a recollection of having obtained urea from the blood of a turkey some few years ago. If no urea exists in the blood of birds, my theory falls to the ground. In this paper, therefore, I have laid special stress on the presence of this principle and the relative amounts contained in the blood of the two classes of animals discussed.

The statement that urate of ammonium is synthetically produced from this urea can easily be shown to be not only possible, but very probable; as likewise can the fact that it is not produced within the blood itself, but is formed, at least chiefly, in the kidneys. We have already spoken of the changes which take place when urea is introduced into the system of birds, whether by the stomach or otherwise, and certain experiments which I have made tend to indicate that glycine is, at any rate, one of the principles which plays a part in the building up of urate of ammonium. In the case of small birds, feeding on canary seed and water, the throwing out of the white urate of ammonia was observed hour by hour, and the quantity noted. When the seed was taken away—water only being left—the amount of the urate rapidly diminished. The exhibition of sugar did not increase the urate, but when a mixture of urea and glycine was given the amount of the urate was rapidly increased, whereas urea given alone at this period of the fasting produced little or no effect in the production of urate of ammonium. I do not, however, lay too much stress on these observations, as they were not sufficiently numerous, nor were the amounts weighed with sufficient accuracy.

From this theory it follows also that, though there is no need for uric acid to be present in the blood of animals which secrete it, the presence of urea is absolutely essential.

One of *my observations* is to this effect:—The blood of a goose

being examined, urea was crystallised out from it. Search was then made for uric acid, but no trace of this substance was discovered, and yet the renal secretion consisted of urate of ammonium.

It has been often said that the reason why uric acid is not found in the blood in any appreciable quantities is owing to the rapidity of its excretion by the kidneys, but this statement should certainly apply to urea, which, as I have shown above, is always found in the blood.

The existence of uric acid in the blood may be looked upon, therefore, as a morbid phenomenon. With birds, and especially those kept for domestic purposes, as, for instance, caged birds or ducks, the water they drink is frequently strongly impregnated with soluble urate, and this, when taken into the alimentary canal, is absorbed into the blood. I was enabled to crystallise uric acid out of the water of a duck pond, as I stated in a paper read before this Society in 1886. On the other hand, I have examined the blood of many ducks without being able to detect a trace of uric acid.

In the human subject, in which the average quantity of uric acid found per diem is small compared with the formation of urea, the blood appears, nevertheless, to be frequently contaminated by its presence, not, however, in the form of urate of ammonium, in which it is doubtless thrown out by the kidneys, but in the form of biurate of sodium, in which shape it exists also in morbid deposits—the so-called chalk-stones of medical authors.

When uric acid is not introduced into the blood by the alimentary canal, its presence must, according to my view, be accounted for by its absorption into the blood from the kidneys after its formation in these organs, and the salt is necessarily changed by the blood from urate of ammonium to biurate of sodium; whereas, according to the old view, it had to be assumed that urate of sodium was converted into some superurate of ammonium, which I believe all chemists would regard as an impossibility.

In conclusion, I may remark that the facts and deductions brought forward in this paper must prove, if established, not only of interest to the physiologist, but must aid the pathologist in the investigation of several diseases and be of value in devising methods for their treatment.

The Society adjourned over the Whitsuntide Recess to Thursday, June 1.

Presents,. May 18, 1893.

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Five Photographs of Nebulæ and Star Clusters.

Dr. Isaac Roberts, F.R.S.

OBITUARY NOTICES OF FELLOWS DECEASED.

JAMES THOMSON, lately Professor of Civil Engineering and Mechanics in the University of Glasgow, was born in Belfast on February 16, 1822.

His father, a mathematician of very high order, was, in the first instance, Mathematical Master and Professor of Mathematics in the Royal Belfast Academical Institution; but in 1832 became Professor of Mathematics in Glasgow University.

James Thomson, and his brother William, Lord Kelvin, entered the classes at Glasgow University at an unusually early age. They were never at school, having received their early education at home from their father. They passed through the University together, both with high distinction, the two lads usually obtaining the first and second prizes in each of the classes they attended.

At a very early age also James Thomson showed evidence of considerable inventive genius. When he was about sixteen or seventeen he invented a mechanism for feathering the floats of the paddles of paddle steamers. Steamboats, even on the Clyde, were comparatively novel in those days; and the invention was looked on with much interest by the Clyde engineers to whom it was shown. Unfortunately, from a commercial point of view, however, it turned out that another method of accomplishing the same object had been invented and patented only a few months earlier.

After passing through the University curriculum, James Thomson took the degree of M.A. with honours in Mathematics and Natural Philosophy at the age of seventeen.

As he had decided on adopting civil engineering as his profession, he went, in the autumn of 1840, to the office of Mr. Macneill (afterwards Sir John Macneill), in Dublin. But unfortunately his health had, shortly before, to some extent, broken down. He was obliged to leave Mr. Macneill's office after about three weeks and return home.

In 1840 a new departure was made in Glasgow University, which proved of great importance, and which has had far-reaching influence on the practical teaching of engineering in this country. This was the foundation, by Queen Victoria, of the first Chair of Civil Engineering and Mechanics in the United Kingdom. The first professor was Lewis Gordon, who was succeeded fifteen years later by Macquorn Rankine. James Thomson, at home and in delicate

health, attended Professor Gordon's classes in engineering, and was busy with inventions of various sorts; and particularly with a curious boat, which, by means of paddles and legs reaching to the bottom, was able to propel itself up a river, walking against the stream.

In 1843 he was able to resume work as an engineer, and he went to Millwall, to the works of Messrs. Fairbairn and Co., of London and Manchester. He was not, however, able to remain with them for the full time of his apprenticeship. Illness returned; he was obliged to go home; and this illness proved the commencement of a period of delicate health which lasted for years, and, indeed, produced a permanent effect on his whole life.

During the months which he spent at Millwall, he was busy with improvements in furnace construction for the purpose of prevention of smoke. The gases of combustion were to be taken downwards through the furnace instead of upwards; and the fire bars were to be tubes with water circulating through them.

After his return to Glasgow he was obliged to confine himself to work which did not involve bodily fatigue. He occupied himself much with invention; and particularly gave his attention to machines for the utilisation of water power.

He constructed a horizontal water-wheel, which he named a Danaide, being an improvement on the Danaide of Manouri Dectot; and somewhat later, after much investigation and research, he invented a wheel which, from the nature of its action, he called the vortex water-wheel. This form of wheel was patented in 1850. It was an important advance on water-wheels of previous construction. The moving wheel was mounted within a chamber of nearly circular form. The water, injected under pressure, was directed, by guide blades, to flow tangentially to the circumference of the wheel; and was led through the wheel to the centre by suitably formed radiating partitions. Thus the water yielded its kinetic energy derived from one half of the fall, and its potential energy from the other half, to the wheel by pressure on the radial partitions, as it passed inwards to the centre, whence it quietly flowed away in the tail-race. A considerable number of these wheels were designed by him for various factories and for different purposes. They were made and supplied by Messrs. Williamson Bros., of Kendal, and gave much satisfaction.

In 1847 his mind was also busy with a question to which at a later date he gave much thought and labour, and to the solution of which he made contributions of great importance. On April 5 of this year there appears a memorandum in his handwriting:—" This morning I found the explanation of the slow motion of semi-fluid masses such as glaciers."

During 1848 his first three important scientific papers were *published.* The first of these was on " Strength of Materials as

influenced by the existence or non-existence of certaiu mutual Strains among the Particles composing them." The second was a remarkable paper on "The Elasticity and Strength of Spiral Springs and of Bars subjected to Torsion." In this paper the action of a spiral spring was explained, and important principles connected with the subject of torsion were brought forward. These papers were published in the 'Cambridge and Dublin Mathematical Journal,' November, 1848.

The third was, perhaps, yet more remarkable. It was contributed to the Royal Society of Edinburgh, and was on "The Parallel Roads or Terraces of Lochaber (Glenroy)." These remarkable *terraces* or *shelves* had attracted much attention. Darwin, Lyell, David Milne, Sir G. Mackenzie, Agassiz, Sir Thomas Dick Lauder, and others had discussed the causes of their formation. James Thomson, however, gave in this paper what is now the accepted explanation.

Curiously, Professor Tyndall seems not even to have known of the existence of the paper when he gave his admirable exposition of this wonderful natural formation at the Royal Institution in 1876. He attributes the explanation of the Parallel Roads to Jamieson, 1863; whereas the whole theory had been given by Thomson in 1848 in the paper just mentioned with details as to necessary climatic circumstances, not noticed by Tyndall.

In January, 1849, he communicated to the Royal Society of Edinburgh a paper of great importance, which was printed in the 'Transactions' of the Society, and was afterwards republished, with some slight alterations by the author, in the 'Cambridge and Dublin Mathematical Journal,' November, 1850. The title of this paper was "Theoretical Considerations on the Effect of Pressure in lowering the Freezing Point of Water." The principles expounded in this paper were afterwards, in 1857, used as the foundation of his well-known explanation of the plasticity of ice, discovered by Forbes; and later, from 1857 onwards, for several years, the whole subject afforded him much food for thought; and extensions and developments in various directions followed. The paper of 1849 was of great intrinsic importance. In it, by the application of Carnot's principle, an absolutely unsuspected physical phenomenon was discovered and predicted, and the amount of lowering of the freezing point of water was calculated. The phenomenon was shortly after experimentally tested and confirmed by his brother, Lord Kelvin.

But the paper has another title to interest, which is not so generally known. In it for the first time Carnot's principle was stated, and Carnot's cycle described, in words carefully chosen, so as not to involve the assumption of the material theory of heat, or rather, as Thomson himself puts it, the supposition of the "perfect conservation of heat."

For the sake of clearness, it may be well to leave here for a moment the chronological order of James Thomson's life, and to explain briefly the subsequent development of the ideas first disclosed in this paper of 1849.

Forbes had discovered, by observations and experiments on the Swiss glaciers, the property of *plasticity* in ice. The fact of plasticity in ice was at first doubted ; but it was afterwards admitted, and various explanations were offered of this property, so remarkable in a brittle and, above all crystalline, substance.

In this connection, Faraday called attention to the freezing together of two pieces of ice placed together in water; and from this arose a partial explanation, by Tyndall, under the designation of " Fracture and Regelation." But the theory, and even the not logical juxta-position of the two words, did not satisfy James Thomson. There was nothing to show why or how reunion (or "regelation") should take place after fracture. He saw, however, that an extension of his own previous principle of lowering of the freezing point by pressure allowed him to apply it to the effect of distorting stress on solid ice, and would give a perfect explanation of all Faraday's observations and experiments on the union and growth of the connecting link between two pieces of ice under water, pressed together by any force, however small.

By this extended thermodynamic principle he also accounted for the yielding of a mass of ice crystals (dry snow, for instance) at *temperatures lower than the ordinary freezing point.* He demonstrated that the mutual pressures must melt the ice at, and close around, the points of contact; and that, when there is relief from the internal stress by this melting, the low temperature of the main solid mass, and the extra cold due to the latent heat required for liquefaction of the yielding portions, cause the melted matter to re-freeze in the places to which it has escaped in order to relieve itself from strain. Thus a complete explanation, based on a demonstrated physical principle, was offered of the phenomenon.

Thomson's explanation did not, certainly at first, commend itself thoroughly to Faraday. A very interesting correspondence between them ensued ; and Faraday made a number of beautiful and interest-ing experiments, with the object of showing that the placing of two pieces of ice on opposite sides of a film of water (between them) would give rise to the conversion of the film of water into ice, and cause the union of the two pieces of ice, the principle being that of the starting of crystallisation in a supersaturated solution by means of a crystal of the solid. James Thomson, however, showed that, in the experiments adduced by Faraday, pressure between the ice blocks *was not absent.* For example, in an experiment in which two pieces *of ice,* with a hole through each, were mounted on a horizontal rod of

glass, he pointed out that the capillary film of water between the slabs draws them together with not inconsiderable mutual pressure, and hence the freezing. Thomson further showed that when two pieces of ice are brought to touch each other at a point wholly immersed under water, and thus free from capillary action, the most minute pressure pushing the two together causes the growth of a narrow connecting neck, which may be made to grow by continued application of the pressure; while the application of the smallest force tending to draw the two asunder causes the neck to diminish in thickness, and finally to disappear.

In later years James Thomson further developed the theory of 1849. He showed that stresses, of other kinds than pressure equal in all directions, can relieve themselves by means of local lowering of the freezing point in ice; and he showed, by theory and by experiment, that the application of stresses may assist or hinder the growth of crystals in saturated solutions. Some of these conclusions are of such importance that they deserve to be better known. The title of the paper in which the last-named results were given is, " On Crystallisation and Liquefaction as Influenced by Stresses tending to Change of Form in the Crystals,"* 1861. It included the amended and extended theory of the plasticity of ice.

In 1850, James Thomson was engaged in perfecting his design for the Vortex water-wheel. He had soon some orders for the wheel; and in 1851 he took the important step of settling down as a civil engineer in Belfast.

His business grew by degrees. His health improved, and we find him occupied in the next two or three years with scientific investigations as to the " properties of whirling fluids." This led to improvements in the action of blowing fans on the one hand, and, on the other, to the invention of a centrifugal pump and to improvements in turbines which were described to the British Association at Belfast in 1852. At this meeting, also, he described " A Jet Pump, or Apparatus for drawing up Water by the Power of a Jet "; and these investigations led to the designing, on the large scale, of pumps of this kind. Some of these pumps have done important work in the drainage of low lands at places where a small stream, capable of supplying the jet, can be found in the immediate proximity. His investigations on the mechanics of whirling fluids, again, led to the design of great centrifugal pumps, the largest of which are now at work on sugar plantations in Demarara.

It will thus be seen that he was giving much attention to water engineering; and in November, 1853, he became resident engineer to the Belfast Water Commissioners, a post which he occupied till the end of 1857.

* ' Roy. Soc. Proc.,' Dec. 5, 1861.

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In this year he was appointed Professor of Civil Engineering and Surveying in Queen's College, Belfast. He became fully occupied with the duties of his professorship, and gave up his office and business as a civil engineer, except for the connection which he retained with some of his former clients, and for business in consultation.

The professorship in Belfast he held till the death of Macquorn Rankine in 1872. By this event the Professorship of Civil Engineering in Glasgow became vacant; and James Thomson was in the next year appointed by the Government to succeed him.

In 1888 his sight unfortunately began to fail; and the malady, from which both his eyes suffered, proceeded so far that it became necessary for him to resign his University work. This he did after the end of the session 1888–89. Happily, however, he retained more or less of his eyesight till the end of his life; and as he became more accustomed to the condition of his eyes he was better able to make use of what remained to him, and was able to move about freely with but little assistance, and even to read and write a little, and to make on a large scale the diagrams which he used to illustrate his Bakerian Lecture on " The Grand Currents of Atmospheric Circulation."

His death was almost sudden and was the beginning of a sadly tragic time in his family. In a single week Professor Thomson, his wife, and youngest daughter were all attacked with cold, which was quickly followed by inflammation of the lungs. The next week saw the death of all three; his daughter surviving him only three days, and Mrs. Thomson seven days. Professor Thomson's death took place on the 8th of May, 1892.

It is not possible in the limits to which this notice must be confined to refer to all James Thomson's papers, nor to give a complete list of the many subjects which occupied his attention.

Already some of his contributions to thermodynamics have been mentioned ; but it must be further remarked that during the portion of his life which was occupied with teaching, he gave great attention to this subject, endeavouring to improve the nomenclature and modes of expression of the various principles and propositions connected with it, and to simplify modes of explanation and of statement.

Another very remarkable contribution to thermal science and thermodynamics was his extension of Andrews' discoveries on the subject of the continuity of the liquid and gaseous states of matter. Thomson's mode of conception of the whole subject, which led to the construction of a model in three dimensions to show the mutual relations between pressure, volume, and temperature of such a substance as carbon dioxide under continuous changes of pressure, and volume, and temperature, was perfectly new and most important. The model itself threw a flood of light on the question and the imagining of the

extension of the three-dimensional surface so as to include an unstable condition of the substance, partially realisable and even well known in the phenomena of a liquid passing its boiling point without forming vapour, and in similar unstable conditions, was an advance in the theory of this important question, the consequences of which are not even now completely realised. The verification of Thomson's theories on this subject has proved a fruitful field of experimental investigation for many workers.

Another subject of great importance to which Professor James Thomson devoted much thought and attention was that of safety and danger in engineering structures, and the principles on which their sufficiency in strength should be estimated and proved. He made more than one weighty communication on this subject to engineering societies; and on his appointment at Glasgow, in 1873, he made it the subject of the Latin address which it is the custom for a newly elected Professor to read to the Senatus of the University of Glasgow. An address in English on the same subject became his inaugural lecture to the students of his class in engineering.

When he took up the question, about 1862, he felt that ordinary engineering practice as to testing of structures, boilers for example, was both illogical and unsafe. He considered that the tests usually applied were quite insufficient to permit of an engineer feeling justified in risking the lives of men and the property of his employers to the dangers of breakdown. It was then a common opinion that severe testing should not be applied lest the structure should be weakened by the test itself; but Thomson denied that the test does weaken the structure if the structure be good; and pointed out that the real reason for not applying a proper test was, frequently, fear lest the structure should be found far inferior in strength to that which it was intended to have. The truth of Professor Thomson's contentions is now admitted by the highest engineers; and the best engineering practice has, happily, undergone a thorough reform in this respect.

Certain geological questions possessed much interest for James Thomson. We have seen how, at an early age, he investigated the parallel roads of Glen Roy; and on many subsequent occasions he examined with great care the places where he chanced to be residing, and found and described glacier markings. He traced out, on more than one occasion, specially interesting features of the ice action, endeavouring to determine, by means of an examination of the markings, details as to the motion of the ice, whether in the form of glacier or in the form of icebergs taking the ground in shallow waters.

His attention was also directed to the jointed prismatic structure seen at the *Giant's Causeway* in Ireland, and elsewhere. No wri-

factory explanation of this remarkable phenomenon had been given. The old theories, involving a supposed spheroidal concretionary tendency in the material during consolidation, seemed quite untenable. He examined with great care the appearances presented in the surfaces of the stones, and concluded that the *columnar* structure is due to the shrinking and cracking during cooling of a very homogeneous mass of material. The *cross joints* he considered to be in reality circular conchoidal fractures commencing at the centre of the column and flashing out to the circumference.

A very interesting subject, and one of very high importance, to which Professor Thomson gave great attention, is the flow of water in rivers. He investigated, with great care, and from a theoretical point of view, the origin of windings of rivers in alluvial plains, and his conclusions were published in the 'Proceedings of the Royal Society,' May 4, 1876. Later in the same year he constructed, in clay, on a table, a model with which he investigated the movements of the different parts of the water in passing round the bends in this artificial river; and, finally, he made a large wooden model of a river flowing on a nearly horizontal bed with many bends and various obstacles. By aid of fine threads, small floating and sinking bodies, and coloured streams of fluid coming from particles of solid aniline dye dropped into the channel, he was able to follow from point to point the movements of the fluid, and thus to give not only beautiful and striking ocular evidence of the truth of his early conclusions, but also to extend his theory. Papers on this subject were communicated to the Royal Society in 1876, 1877, 1878. The paper of the last-named date was entitled "On Flow of Water in Uniform Régime in Rivers and in Open Channels generally." It contains a very clear and striking account of what does occur in the motion of a river down its inclined channel; and, in particular, of the fact which seems to be ascertained, that the forward velocity of the water in rivers is, generally, not greatest at the surface with gradual abatement from surface to bottom (as would be required under the conditions supposed in the laminar theory); but that, in reality, the average velocity down stream is greatest at some depth below the surface, from which, up to the surface, there is a considerable decrease, and down to the bottom a much greater decrease. This phenomenon he showed very clearly to be due to the rising, of masses of slow-going water, from the bottom, on account of directing action of bottom obstacles. These masses of slow-going water, when they reach the top, spread themselves out, and, mingling with the quicker surface water, give to it, on the whole, a less rapid movement than it should otherwise possess. The paper, as a whole, forms a masterly exposition of this important subject.

Finally, in this brief summary must be mentioned the paper which

was made the Bakerian Lecture for the year 1892. In 1857 Professor Thomson read a paper to the British Association, on "The Grand Currents of Atmospheric Circulation." It appears that his attention was first called to this subject when, during the period of his early delicacy, his father asked him to look into the question of the Trade Winds and write a short account of this atmospheric phenomenon for a new edition of Dr. Thomson's 'Geography,' which was then in preparation. This was done; but young James Thomson found so little satisfaction in the information and theories which he then studied for the purpose that his mind was keenly directed to the question; and in 1857 he himself had formed a theory which he expounded to the British Association.

The subject was before his mind during the rest of his life; and though on account of other pressing work the complete publication of the theory was from time to time deferred, yet it was always his intention to return to the question. When in the last years of his life the affliction of partial blindness came upon him, and when he had somewhat recovered from the first depressing effects of finding himself thus sadly crippled, he set himself in his enforced leisure to complete this work, and, with the assistance of his wife and daughters, to produce the important paper which was read before the Royal Society on the 10th of March, 1892. In this paper a historical sketch is given of the progress of observation and theoretical research into the nature and causes of the trade-winds and other great and persistent currents of atmospheric circulation. Previous theories are discussed and criticised and their merits duly recognized, the theory of Hadley, in particular, being shown to be substantially true. A much more complete theory is then expounded in full detail; and charts and diagrams in illustration show the nature of the aërial motions.

Here this memoir must close. There are many papers of Thomson which have not even been alluded to in it. Nor is it possible or necessary for the present purpose to refer to all the subjects to which his ever active mind directed itself. A character so truly philosophic it is very rare to meet. His was a singularly well ordered and well governed mind. It was, if one may venture to say so, almost too philosophical and too well governed for the business of every-day life. He could scarcely realise a difference between greater and smaller error or untruth. Great or small error and untruth were to be condemned and resisted; and, perhaps, in the matter of public business and in this hurrying nineteenth century pressure, there were those who, thoroughly conscientious themselves, could not yet feel perfect sympathy with his extreme and scrupulous determination to let nothing, however small, pass without thorough examination and complete proof. To temporise was not in his nature; and this

extreme conscientiousness gave rise to a want of rapidity of action which was perhaps the cnly fault in a singularly perfect character.

Purity and honour in word and deed and thought, gentleness of disposition, readiness to spend his labour, his time, his mental energies for others, and for the good of the world in general, all were conspicuous in his life both in public and in private.

Professor Thomson was elected Fellow of the Royal Society in 1877; and he received the honorary degree of D.Sc. from the Queen's University in Ireland, and of LL.D. from his own University of Glasgow, and from the University of Dublin.

In 1853 he married Elizabeth Hancock, daughter of William John Hancock, Esq., J.P., of Lurgan, Co. Armagh, a lady who devoted herself to every minutest interest of her husband's life. They had one son and two daughters, of whom the son and elder daughter survive.

J. T. B.

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